VOL. 257 DECEMBER 1959 No. 10

American Journal of Science

Established in 1818 by Benjamin Silliman

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Published 10 times a year. Ten dollars a year. One dollar twenty-five cents a copy.
Foreign, ten dollars sixty cents. Canada, ten dollars thirty-five cents.

AMERICAN JOURNAL OF SCIENCE

Established in 1818 by Benjamin Silliman

THE FIRST SCIENTIFIC JOURNAL IN THE UNITED STATES

DEVOTED TO THE GEOLOGICAL SCIENCES AND TO RELATED FIELDS

Editors: JOHN RODGERS—JOSEPH T. GREGORY
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Entered as second-class matter at the Post Office at New Haven, Conn., under the Act of March 3, 1879. Published monthly, except July and September, at Sterling Tower, Yale University, New Haven, Conn.

Subscription rate $10 per year ($1.25 a number). Student rate $5 per year (application blanks available on request). Postage prepaid to the United States; 35 cents per year to Canada; 60 cents per year elsewhere.

The JOURNAL completed its *first* series of 50 volumes in 1845, its *second* series of 50 volumes in 1870, its *third* series of 50 volumes in 1895, its *fourth* series of 50 volumes in 1920, its *fifth* series of 36 volumes in 1938. Since 1938 the numbering by series has been discontinued, and volumes are annual and bear the whole numbers as of 1818, that for 1939 being volume 237.

Back numbers and volumes, also cumulative indices of all series, may with few exceptions be obtained from the office of the AMERICAN JOURNAL OF SCIENCE, Sterling Tower, New Haven, Conn. Positive microfilm copies of each complete volume may be purchased by subscribers to the paper edition from *University Microfilms*, 313 N. First St., Ann Arbor, Michigan. Regular subscribers may purchase each past year in microcard form, from *The Microcard Foundation*, Wesleyan University, Middletown, Connecticut.

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Suggestions to Authors of the Reports of the United States Geological Survey, 5th ed. Washington, D. C., 1958 (Government Printing Office, $1.75).

American Journal of Science

DECEMBER 1959

SYMPOSIUM ON CONCEPTS OF STRATIGRAPHIC CLASSIFICATION AND CORRELATION

W. CHARLES BELL, GROVER E. MURRAY, L. L. SLOSS, Co-CHAIRMEN

INTRODUCTION

This symposium originated during the annual meeting of the Research Committee, Society of Economic Paleontologists and Mineralogists, St. Louis, Missouri, 1957, when L. L. Sloss suggested that the annual symposium of the Committee for 1959 be devoted to a review and discussion of the basic concepts involved in stratigraphic classification and correlation. Subsequently, W. C. Bell and L. L. Sloss agreed to serve with G. E. Murray, Chairman of the S. E. P. M. Research Committee for 1959, as Co-Chairmen for the symposium. It was organized and held in Dallas, Texas, in conjunction with the A. A. P. G. —S. E. P. M. annual meeting as an all-day session of papers followed by an open, panel discussion in the evening.

The S. E. P. M. Executive Council and Publications Committee subsequently voted not to publish the papers as a unit and to release them for individual publication. John Rodgers and Joseph T. Gregory, Editors of The American Journal of Science, had previously requested permission to publish the papers and so, with release by the S. E. P. M., they invited each participant to submit a limited version of his paper for group publication in the Journal. The results are presented here.

The idea behind the development of such a symposium was to not only to review the concepts and principles involved but also to focus attention on the different approaches to classification and correlation, including nomenclature, employed by various workers. In the selection of speakers and their topics we attempted to maintain a reasonable balance between (1) classic and new approaches to the problem, (2) strata of various ages, and (3) geographic areas (geologic provinces). We hope that the results prove of value not only to geologists in general but also to commissioners of the American Commission on Stratigraphic Nomenclature in their current attempts to solidify the language of a new Stratigraphic Code. General agreement among us as to the nomenclature we shall apply to stratigraphic units is long overdue. Conceptually, we appear to think largely in terms of the same kinds of stratigraphic units and, obviously, we use the same units in our everyday work. However, for reasons not wholly justifiable we have not yet individually agreed to call them by the same names.

A formation by any other name will map as well.

[American Journal of Science, Vol. 257, December 1959, P. 674-683]

TOWARDS HARMONY IN STRATIGRAPHIC CLASSIFICATION

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ABSTRACT. Stratigraphic classification is the systematic zonation of the strata of the earth's crust with reference to any of the many different properties or attributes which rock strata may possess. It is an essential step for us, both toward the scientific and philosophic understanding of the rocks of the earth's crust and toward their economic utilization.

This concept of stratigraphic classification is extremely simple, and offhand it is hard to see why there should be any problems other than the strictly geological ones involved in the accurate scientific identification of stratigraphic characters and the selection, tracing, and definition of units based on variations in these characters. That there are other problems, and even some degree of confusion and controversy, seems to result principally from the following:

1. Lack of a clear concept of what is being classified.
2. Lack of adequate standards of reference for specific stratigraphic units.
3. Attempts to zone strata according to two or more different characters using only one set of units.
4. Lack of precise and uniform terminology.
5. Proliferation of unnecessary terms.
6. Influence of preconceived notions on stratigraphic classification handed down from the early stages of the development of stratigraphy.
7. Intolerance by specialists in one field of stratigraphic classification toward potential contributions of other fields.

Great progress is being made toward general agreement on principles of stratigraphic classification and toward uniformity of usage in terminology. The work of the International Subcommission on Stratigraphic Terminology, as well as that of the several national commissions on stratigraphic nomenclature is showing excellent results. Recent questionnaires distributed to stratigraphers throughout the world, as well as other samplings of thought and opinion, reveal a very encouraging trend toward harmony on points of stratigraphic classification and terminology which only a short time ago were confused by very diverse viewpoints. A summary of the conclusions arising from 75 replies to a recent world-wide questionnaire of the International Subcommission on Stratigraphic Terminology is particularly illuminating. Some areas of controversy still remain, but the goal of reasonably close international accord appears definitely attainable. An interesting variation in views on stratigraphic classification lies in the concept of the USSR Stratigraphic Commission of a single kind of stratigraphic classification resulting in a single set of units based on natural stages in the development of the earth's crust, in contrast to the multiple kinds of stratigraphic units to which most of the rest of the world appears to subscribe.

Stratigraphic classification is the systematic zonation of the rock strata of the earth's crust with reference to any of the many different properties or attributes which rock strata may possess. It is an essential step for us, both towards the scientific and philosophic understanding of the rocks of the earth's crust, and toward their economic utilization. It is a manifestation of our almost instinctive recognition that the best approach to understanding is by means of analysis—by breaking things down into their component parts whenever we cannot adequately grasp them as a whole—and in order that eventually we may more truly grasp them as a whole.

Stratigraphic classification thus involves, firstly, the analysis of strata with respect to the distribution of any selected property or attribute (lithology, chemical content, fossil content, etc.) and, secondly, the grouping of strata into unit bodies, each representing in itself a more or less similar or unified development of the property or attribute in question, and each bounded by varia-

tions in the presence or development of this property or attribute. (A hierarchy of units, that is, a classificatory system with units of differing rank, may be usefully employed for certain properties or attributes to express different degrees of magnitude or different degrees of detail in uniformity with respect to the quality in question.) A high purpose of stratigraphic classification is thus to be able to recognize the variations in any of the many properties and attributes of the rocks of the earth's crust, to be able to mutually communicate regarding these variations, and to be able to employ them to the best advantage, scientifically and economically. Only if all rocks were absolutely uniform in all characters, including mode of origin and age of formation, would there be no need for stratigraphic classification.

This concept of stratigraphic classification and its purpose is exceedingly simple, and off-hand it is hard to see why there should be any problems or controversies about it, other than the strictly geological or technical ones naturally involved in the accurate scientific identification of stratigraphic characters and in the selection, tracing, and definition of units based on variations in these characters. That there have been other problems, and perhaps even some degree of confusion and controversy, seems evident from the literature and is perhaps even indicated by the call for the present S.E.P.M. symposium on Concepts of Stratigraphic Classification and Correlation. It may be worth while therefore to explore with you what, in my opinion, are probably the principal reasons for such disagreements as may exist.

1. A common source of confusion is the lack of a clear concept of what is being classified. I believe that most of us agree that there may be many different lines of stratigraphic classification and many different kinds of stratigraphic units, depending on the particular characters or attributes of rock strata which are under consideration. Thus rocks may be classified stratigraphically according to their lithologic character, according to fossil content, according to mineral character, according to geologic age or time of origin, according to chemical properties, according to seismic properties, according to environment of origin, and according to numerous other properties which vary through the sequence of strata making up the earth's crust. Some categories of classification are, of course, vastly more important and more useful than others. Perhaps the most of us will need to use only one or two or three or four of these, but let's be broadminded enough to recognize that others do exist which may be usefully employed for certain purposes. No one need use any which do not appear useful to him. Obviously, however, if a number of people, scattered widely over the globe, are going to be engaged in classifying strata into units, it is necessary that it be clear to each just exactly what property or attribute the other is classifying in any particular case and what the units which each has named represent, before anyone else can hope to intelligently use these units. When a unit is employed, therefore, it should be clear whether this unit is a body of strata representing a certain lithology, a certain assemblage of fossils, a certain environment of deposition, the range of a certain fossil, a certain interval of geologic time, or whatever else; since unless this is done, it becomes completely impossible for anyone else to even begin to recognize, identify, or trace this unit elsewhere.

2. A second source of confusion is a lack of adequate standards of ref-

erence for specific stratigraphic units. Admittedly it is often difficult to describe in words adequately the feature or features on which a unit is based and hence the stress which must be placed on *standard reference sections* of rocks for the definition of most kinds of stratigraphic units. This is the most stable anchor which we can have in this oft-disturbed sea of stratigraphic classification. It is as essential to stratigraphic classification as types are to biologic classification, or as Bureau of Standards references are to physical measures. I have emphasized previously the almost ridiculous situation in our science where tons of paper and innumerable man-years have been wasted in futile controversies about where to draw boundaries between certain series, certain stages,—when no type or standard section of rocks has ever been established or accepted as a reference for these units. Here is a crying need with respect to world-wide chronostratigraphic (time-stratigraphic) classification—the need for establishment, by some internationally recognized authority of standard reference rock sections for our major time-stratigraphic units.[1] A similar need for standard reference sections exists for most other kinds of stratigraphic units.

3. A third source of confusion lies in the frequent attempts to zone strata according to *two* or more different characters using only *one* set of units. This might be considered funny if it weren't so tragic. Is it possible that anyone would try to set up standards or units for the measurement of two different properties using only a single set of units? Would you try to measure both weight and distance in terms of pounds? Or count the different fruits in a mixed fruit basket in terms of oranges? Well, maybe not, but some have proposed that a lithologic unit—a formation—should not be differentiated from a time-stratigraphic unit. Others have proposed that the same set of units should be used for biostratigraphic divisions as for time-stratigraphic divisions, inferring that fossil zones are the only time-stratigraphic units. There is here a

[1] Some have proposed that the standards of reference for chronostratigraphic units should be defined in terms of biologic features or stages in biologic evolution. However, such features, divorced from specific rock sections, cannot in themselves constitute adequate basic standards of reference for chronostratigraphic units since we are not justified in assuming, (a) that the chronostratigraphic range of any biologic feature will ever be fully known, (b) that even when known such range will be the same over the whole world through all different facies developments of rock strata, (c) that evolution in all lines or in any one line proceeded everywhere at the same pace or in exactly the same direction, (d) that the first or the last known occurrence of any biologic feature in any one region marks the same point in geologic time as the first or last known occurrence in all other regions, and (e) that the age according to such biologic features will accord with absolute age determinations which may eventually be worked out by radioactive methods. The fallacy of such biologic standards for chronostratigraphic units is readily seen in the obvious fallacy of the proposed designation of the base of the Cambrian system (a supposedly isochronous surface) as coinciding with the first known occurrence in any specific region of fossil evidences of organized life!

The only reliable objective standard to which we can go back for reference with respect to any chronostratigraphic unit is a designated sequence of strata and the scope in geologic time which it represents. Some have objected that unconformities with their corresponding hiatuses in geologic time may exist in such sequences. However, probably most sequences of strata include some hiatuses in deposition, if we accept a short enough time interval as a hiatus, but this does not affect the over-all span of time represented by the sequence of strata. Desirably, for purposes of ease and accuracy in extension and identification elsewhere, the standard section of a chronostratigraphic unit should be as complete as possible and should carry as many useful criteria (biologic and other) for time correlation as possible. Desirably also, the top and the bottom of the standard section should not be marked by unconformities but should be horizons in a continuously deposited sequence of sediments.

fundamental lack of comprehension that, however close the relation, we are dealing with two intrinsically different things. Surely, the weight of a pile of bricks in pounds may be proportional to its height in feet and the age of a sequence of strata may be determined by its correspondence to a certain fossil zone, but this is not always so, nor inevitably so, and there is no more necessary relation between fossil content and age than between weight and height. Nothing useful is lost and much confusion is avoided if we keep distinct characters separate in our classificatory systems. If we are talking about the age of strata, let's say so; if we are talking about their fossil content, let's say so. They are not the same thing.

A particularly common source of confusion results from the attempt to express with one and the same set of units both a physically discernible character and a feature of a purely interpretational nature. Thus the determination that a certain body of strata is characterized by a certain fossil assemblage is a relatively stable observational fact. The conclusion that this fossil assemblage indicates a certain geologic age or a certain environment of deposition is more a matter of interpretation which may vary considerably depending on individual viewpoint or may change drastically with the general development of our knowledge. Fossil content, age, and environment are three different things. Each of these may bear closely on the others, but each represents a separate property or attribute of the strata which cannot necessarily be expressed by the same set of units as the others. As another example, I will admit that along with many others I used to think of a formation as both a lithologic and a lithogenetic unit. I am now convinced that strictly speaking we are not justified in combining even these two concepts, one dominantly objective and the other dominantly subjective, to be expressed by a single type of stratigraphic unit. Lithogenesis may commonly control physical lithology, and lithogenetic evidence may commonly be an important aid to choice of boundaries and to tracing of a lithologic unit, but fundamentally they are two different things and units based on mode of genesis will not necessarily coincide with those based on observed lithology or vice versa.

Another interesting example of confusion resulting from lumping two concepts under one set of terms is evident in the common usage of the term "fossil zone," *Fulanus smithi* Zone for example. Thus, one group of paleontologists would interpret *Fulanus smithi* Zone as the body of strata characterized by a certain assemblage of fossils of which *Fulanus smithi* happened to be a prominent member. Another group would understand *Fulanus smithi* Zone to mean the total body of strata in which the species *Fulanus smithi* occurred regardless of its associates. (Moreover, in neither group would there be uniform opinion as to whether actual specimens of either *Fulanus smithi* or the assemblage fossils would have to be present for strata to be included in the zone, or whether simply supposed time equivalence would qualify strata for inclusion.) The two concepts are both quite useful, though quite different, but it is most interesting that supporters of each seem almost outraged to think there could be any other interpretation than their own. Most people queried on the point admit, as I do, that you just can't tell what is meant by *Falanus smithi* Zone. The need for distinct terms to separate the two concepts—a body of strata unified by the occurrence of a certain *assemblage* of fossils and a body

of strata unified by the stratigraphic *range* of a certain fossil form—seems obvious, and to fill this need the terms *assemblage-zone* and *range-zone* have been proposed.

4. A fourth source of confusion is the lack of precise and uniform terminology. Once concepts are clear we then need a precise and uniformly accepted terminology to express them. Much time which could profitably have been spent on more important problems of stratigraphy has been wasted through the use of vague or variably defined terms. Here arises the question of whether or not we should necessarily stay with old terms which were originally poorly defined, which are presently used with quite variable significance, and which are no longer adequate for our needs. In my opinion all language is dynamic and should be the servant rather than the master of those who use it. Whether an old term should be revived, re-defined, or replaced is largely a matter of how best the interests of communication and understanding can be served in any particular case. I do not feel there is anything sacred about mere priority of usage—even that of an International Geological Congress 60 years ago.

An example of a term which, in my opinion, has become so variable in meaning as to have lost much of its usefulness is the term *biozone*. It was originally defined by Buckman (1902) as a *time* term to express the time during which a fossil form existed. Some still follow this usage. Others use biozone to refer to the total body of strata in which a certain form occurs. Still others use biozone to refer to all strata of the same age as the life-span of the certain fossil form regardless of whether the form itself is present in these strata or not. Because of the vague significance of biozone, the fact that almost nobody uses it in the sense of its original definition, and because of the ingrained nature of other current discrepant usages of the term, I would strongly favor replacing it by the more descriptive term *range-zone*, defined as the body of strata characterized by and representing the total range of occurrences of some one particular species, genus, or other taxonomic entity. I believe this is more practicable than any attempt at redefinition of biozone, and I would suggest that those who wish to express clearly the concept defined in the preceding sentence will find range-zone a more useful and less equivocal term than biozone.

5. A fifth source of confusion results from the proliferation of unnecessary terms. Too many terms for stratigraphic units may have as bad or worse consequences than too few. Simplicity of terminology is an invaluable advantage if it can be attained without sacrifice to understanding. I see no reason why this cannot be done.

The word *zone* is in common use in many languages as a general term for a stratigraphic unit of any kind. It is defined with remarkable adequacy for stratigraphy in even so general a reference as Webster's Dictionary, as "a belt, layer, or series of layers of rock . . . characterized by some particular property, action, or content." Thus we commonly speak of lithic zones, mineral zones, fossil zones, marine zones, calcareous zones, sandy zones, shaly zones, tar zones, oil-producing zones, etc. Zone is a simple and useful word for this general purpose. With an adequate prefix, or adjective, or modifying phrase, to indicate the particular classificatory category in which it is being used, it can satisfactorily serve as a basic unit for most kinds of stratigraphic classifi-

cation. Larger or smaller scale units can be called super- or sub-zones. Some would, of course, restrict zone to biostratigraphic classification, but I see no reason for such restriction. Biostratigraphic zones are clearly differentiated from others by the fossil name preceding the term or merely by the prefix, as fossil-zone, fossil assemblage-zone, fossil range-zone, *Didymograptus* Range-zone, etc.

I must confess some dismay at the number of stratigraphic terms with which the literature has been flooded. If a new term helps to make an important concept clear, I am in favor of it, but it seems to me that many of the terms which have been introduced, instead of clarifying a possibly valuable thought, serve rather to hide it behind a strange and unnecessary name, and thus tend to obscure rather than to illuminate.

Thus, what is a Teil-zone? It is supposed to be the range-zone of a fossil at some particular locality as distinguished from its total range-zone. Well, for this concept to have any meaning you have to name the locality and if you do that, why isn't range-zone on the Rocky River section just as simple as Teil-zone on the Rocky River section and a lot more expressive? I myself have not found too much use for such terms as Teil-zones, tectozones, lithostromes, biostromes, topozones, formats,[2] monothems, geoliths, bioliths, sequences, etc., although I have no particular objection to them if anyone finds them useful.

6. A sixth source of confusion and controversy lies in preconceived notions on stratigraphic classification handed down from the early stages of the development of stratigraphy. Under this heading I refer particularly to certain inheritances from the early history of stratigraphy which, even though they may be no longer generally accepted, still impede our present procedures in stratigraphic classification. This is, of course, no criticism of past workers, but only an indication of the progress of the science. Probably what we arrive at today will stand no better in the future.

As an example is the tendency to believe that the classic time-stratigraphic divisions (systems, series, stages) established largely in Europe during the last century constitute "natural divisions" of the earth's stratigraphic column which can be recognized as such around the world. This is, of course, an inheritance from the early influence of Cuvier, d'Orbigny, and others, who interpreted the local breaks in faunal successions due to unconformities or changes of environment as evidence of world-wide catastrophes which drastically changed the composition of the earth's organic life. Although few would now openly subscribe to this extreme segmentation of the stratigraphic record, nevertheless many almost unconsciously endow the boundaries of these original time-stratigraphic units with a world-wide significance far beyond their real nature of quite arbitrary, though reasonably satisfactory and convenient, divisions of the more or less continuously developing record contained in the earth's sedimentary strata.

7. Finally, among unnecessary sources of confusion has been the intoler-

[2] A *format* has been defined as the body of strata between two marker beds. It seems to me that if this body of strata has a unified lithology, differentiated from that above and below, then it is simply a lithostratigraphic unit; if the marker horizons are time horizons or closely approach time horizons, then the intervening body of strata is a chronostratigraphic unit—an informal stage or substage perhaps. If the body of strata has none of these or other unifying attributes then of what use is it and why name it?

ance by specialists in one field of stratigraphic classification toward potential contributions of other fields. Here I am speaking particularly about those *few* biostratigraphers and paleontologists who seem to feel that paleontology should have a monopoly on time-stratigraphic classification and age dating of rocks. They apparently believe in the closed shop and no painter or carpenter or teamster ever more jealously tried to restrict his trade to union labor than do such paleontologists try to limit age dating and time-stratigraphic classification to paleontologic criteria.

This again is evidently a heritage from the past and the early days of stratigraphy. It is definitely out-moded and out-dated now. The potential contribution of the paleontologist to stratigraphy is greater now than ever before; in dating, in correlation, in ecologic interpretation, and in other ways. But we now see many other keys to local time-stratigraphic classification and, through radio-active determinations, even to geochronology. With everything we have, our precision is still far from satisfactory and certainly our system of time-stratigraphic classification should not be tied inflexibly to paleontology, but should leave the way open to use everything we can. The evolutionary sequence of fossils may always be superior to any other means for geochronologic dating of fossiliferous sediments, but we already know that other methods can contribute greatly to dating and to time-stratigraphic correlation even in the fossiliferous rocks, and certainly they provide the only hope for the great mass of non-fossiliferous or poorly fossiliferous strata.

So much for some of the sources of confusion, and even controversy, with respect to stratigraphic classification. In general, however, great progress is being made towards agreement on principles of stratigraphic classification and towards uniformity of usage in terminology.

Recently, through the International Subcommission on Stratigraphic Terminology, I have received replies from some 75 stratigraphers all over the world and representing some 40 different countries, to a rather comprehensive questionnaire on stratigraphic principles and terminology. (Thirty of the answers to the questionnaire were from members of the Subcommission and most of the other 45 were from members of the International Commission on Stratigraphy.) The results are very encouraging.

Obviously, stratigraphic principles and even stratigraphic terminology are not subjects which can be legislated or decided by vote and a mere preponderance of affirmative or negative answers to a question does not make the answer right or wrong. At the same time, considering the composition of the group polled, I do feel that the results carry considerable weight. I have therefore attempted to summarize briefly for you a few of those conclusions, supported by at least 80 percent of the replies, which seem particularly important or which indicate impressive uniformity of opinion on points which only a short time ago might have seemed highly controversial. I must, however, emphasize that the consensus shown here is only that of a particular group of stratigraphers and that while 40 countries are represented in the replies, this representation is by no means proportional to the geological population of the countries involved, nor can the replies from any one country necessarily be considered as representing the mass of opinion in that country. Moreover, as I will discuss later, the indicated degree of unanimity of opinion is largely for answers from

countries outside of the USSR. My summary of a few of the conclusions suggested by the results of the questionnaire is as follows:

1. It is highly desirable to work toward international uniformity in stratigraphic classification and terminology. The establishment of a new international code under the auspices of the International Geological Congress should be an objective.
2. Stratigraphy deals with the form, arrangement, distribution, origin, and chronologic succession of rock strata. All classes of rocks—igneous and metamorphic as well as sedimentary—fall within the scope of stratigraphy and stratigraphic classification.
3. Stratigraphic classification is the grouping of rock strata according to normal stratigraphic sequence into units with reference to any or all of the many characters, properties, or attributes which rocks may possess. There are thus many kinds of stratigraphic units. Of these the most commonly used are lithostratigraphic, biostratigraphic, and chronostratigraphic units. Table 1 summarizes a recommended scheme of classification and terminology.
4. The term *zone*, adequately prefixed or modified, appears to be the most acceptable term for a general basic unit in all kinds of stratigraphic classification, and particularly in those kinds which do not already have more specialized unit terms.
5. The designation of specific type or reference sections of rock strata which may serve as standards of reference is essential to the adequate definition of almost all kinds of stratigraphic units.
6. A *lithostratigraphic* unit is a body of rock strata which is unified with respect to adjacent strata by consisting of a certain lithologic type or combination of lithologic types. The units of lithostratigraphic classification in order of descending rank are *group*, *formation*, *member*, and *bed*, (or their translated equivalents in other languages). The extent of a lithostratigraphic unit is controlled by the extent of the definitive lithologic features on which the unit was based in its standard reference section. Its boundaries may cut across time horizons, across the limits of fossil ranges and across the boundaries of any other kind of stratigraphic unit.
7. A *biostratigraphic* unit is a body of rock strata which is unified by certain features of its fossil content. There are two principal kinds of biostratigraphic units: the *assemblage-zone* or *cenozone*, consisting of the body of strata characterized by a certain assemblage or association of fossil forms; and the *range-zone* or *acrozone*, consisting of the body of strata representing the total range of occurrence of some one particular species, genus, or other taxonomic entity. Both the assemblage-zone and the range-zone are limited in extent to the actual known occurrences of the assemblage or taxonomic entity concerned. The term *zone*, if standing alone without prefixes, is inadequate for more than very general usage in biostratigraphic terminology since it does not differentiate between the assemblage and the range concepts. The boundaries of biostratigraphic zones may cut across time horizons,

TABLE 1
Stratigraphic Classification

Characters or Attributes of Rock Strata	Units	
	Informal	Formally Named
Lithology (Lithostratigraphic Classification)	zone bed (s)	Group Formation Member Bed
Fossil Content (Biostratigraphic Classification)	zone	Assemblage-zone Range-zone
Geologic Age (Chronostratigraphic Classification)	stage	System Series Stage Substage
Mineral Content	zone	Zone
Other Characters (Chemical, Electrical, Seismic, Environmental, etc.)	zone	Zone

Sub-zones and Super-zones may be recognized.

across formation limits, and across the boundaries of any other kind of stratigraphic unit.

8. A *chronostratigraphic* unit is a body of rock strata which is unified by representing the rocks formed during a specific interval of geologic time. The boundaries of a chronostratigraphic unit as they are extended away from its type or reference section are by definition isochronous surfaces (i.e., surfaces having equal time value everywhere). While fossils, lithology, radioactive data, unconformities, regressions and transgressions, and other stratigraphic criteria serve as our only guides to approximating the correct position of such isochronous bounding surfaces, these surfaces and the chronostratigraphic units which they delimit are fundamentally independent of all physical bases for stratigraphic subdivision and may cut across the boundaries of any of the other kinds of stratigraphic units.
9. Fossils provide one of the most useful criteria of geologic time, and particularly of geochronology, but many other criteria are also usefully employed in chronostratigraphy, such as, observed sequence of beds, tracing of bedding planes, ash falls, lava flows, bentonites, paleoclimatic changes, etc. The progress in radioactive methods of dating stratified rocks is particularly promising and is such as to justify adherence to chronostratigraphic principles which will be compatible with such absolute age dating as well as with methods of relative dating.
10. Although fossils play an extremely important role in chronostratigraphy, there is a fundamental difference between chronostratigraphic units and biostratigraphic units, and the terms used for these units should be distinct one from the other. A chronostratigraphic unit may frequently coincide in its type section with the scope of a biostratigraphic unit but the extent of the biostratigraphic unit away from the

type section is limited to, and defined by, the extent of the fossil occurrences considered diagnostic in the type section, whereas the chronostratigraphic unit extends geographically to include all strata anywhere having the same age as the unit in the type section, regardless of what their fossil content may be.

11. The generally accepted hierarchy of chronostratigraphic terms in descending order of rank is as follows: *system*, *series*, *stage*, and *substage* (or their translated equivalents in other languages).
12. It is considered desirable that competent committees be set up under the aegis of the International Geological Congress to attempt the task of designating specific standard reference stratigraphic sections for each of the systems, series, and stages, without which these cannot have adequate meaning.

In conclusion, with respect to the results of the questionnaire, attention should be called to an interesting variation in stratigraphic philosophy between stratigraphers in the USSR and those of much of the rest of the world. According to the USSR Stratigraphic Commission there is only a *single* kind of stratigraphic classification. This portrays "natural" stages in the development of the earth's crust and the evolution of the organic and inorganic world. Stratigraphic classification is seen by the USSR Commission as the hierarchical grouping of sedimentary, metamorphic and igneous rocks into a single set of units with relation to "natural" steps in the historical development of the earth. The USSR Stratigraphic Commission does not recognize several distinct kinds of stratigraphic units, such as lithostratigraphic, biostratigraphic, and chronostratigraphic units, but considers the criteria for these only as aids in arriving at the single scale of "natural" stratigraphic units. This single scale comprises a hierarchy of units which in order of descending rank are given names which would translate to the English terms: group, system, section, stage, and zone.

It appears to me that the difference in concept of stratigraphic classification reached by the USSR Commission as compared with that of most of the rest of the world lies principally in a different route of approach. The USSR Stratigraphic Commission would appear to start with the assumption that there are "natural" steps or stages in the historical development of the rocks of the earth's crust and that all of the physical and organic characters of the rocks when adequately worked out must accord with these "natural" divisions. Most of the rest of the world, I believe, would favor taking a more objective approach, classifying strata independently along various different lines of observed or interpreted characters without the perconceived conclusion that these would all accord with any "natural" over-all grouping of strata. Our course is inductive; the Russian course is deductive. Since ours is the more elementary and conservative approach, I am hopeful that in the interests of international harmony the USSR Commission may be willing to go along with us even though they may feel that we are groping with too cautious steps toward a conclusion which they have already attained.

[American Journal of Science, Vol. 257, December 1959, P. 684-691]

THE MEANING OF CORRELATION

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ABSTRACT. The word *correlation* ordinarily means simply mutual interdependence or interrelation, but when it was introduced into stratigraphy in the later 19th century, it was used to mean the process of determining the time relations of strata. More recently, a broader meaning has been advocated—determining equivalency or continuity in lithology or fauna as well as time relations. These different equivalencies do not necessarily agree, because of facies relationships like transgression, but the criteria used to establish them overlap broadly, and ultimately the acceptability of such criteria depends on their validity in establishing the true time relations. Hence, in stratigraphy, the older narrower definition of correlation should be retained.

A talk that hinges only on the proper use of a word is in grave danger of being both dull and pedantic. Certainly, discussions of real scientific results are far more interesting and entertaining than discussions of words, but not necessarily more important. For we have to express our scientific results in words, and if the words are ambiguous and mean different things to you and to me, they will fail to transmit the results between us. Accordingly I welcome this opportunity to discuss one such word, to explore its current ambiguity as fairly as I can, and yet finally to present, as though it were the logical conclusion, the view I have held all along.

A paper like this loses all its value if it fails to be clear and unambiguous. In my effort to make my meaning as clear as possible, I have been greatly aided by critical reading of the manuscript by the co-chairmen of the symposium, W. Charles Bell, Grover E. Murray, and Laurence L. Sloss, and by my colleagues Carl O. Dunbar, Joseph T. Gregory, John E. Sanders, and Karl M. Waagé. This does not signify of course that they necessarily endorse all or any of my conclusions.

GROWTH OF THE CONCEPT

The word *correlation*, which comes from a Medieval Latin neologism, has wide currency as a technical term in science, but its meaning in stratigraphy is rather specialized and would not be readily guessed by someone unfamiliar with the jargon of the subject. The general scientific usage derives from the grammatical and philosophical meaning of the word—mutual interdependence or interrelation—and reaches its most precise form in statistics, where it is quantified to describe the degree of interdependence of two (or more) variables.

Like many geologic words, correlation in its stratigraphic sense does not seem to have been deliberately introduced but to have crept into use gradually. The Oxford English Dictionary records its use in approximately this sense by R. I. Murchison in 1849, but it was at least very uncommon down to about 1880. In 1888, J. W. Powell, as Director of the U. S. Geological Survey (Powell, 1889, p. 16-17), announced a plan for a series of essays on the "correlation" of the geologic formations of the different systems, and these were published in 1891 and 1892 as Survey Bulletins 80 to 86 under the title Correlation Papers. Powell did not define his use of the word, but he coupled it with "synchrony" and evidently he meant the essays to be concerned with the

time relations of the formations. In 1895, J. D. Dana adopted the term in the 4th Edition of his Manual of Geology (Dana, 1895, p. 398, 399), defining "correlate strata" as strata "of the same geologic horizon" (in previous editions he had used the phrase "equivalent strata") and giving a list of "means of correlation"—that is to say, criteria for determining equivalence in age. (By "geologic horizon" he evidently meant about what we would now call time-stratigraphic unit.) During the next decades the word appeared (undefined) in the widely used elementary textbooks by Chamberlin and Salisbury and by Pirsson and Schuchert, "correlation charts" became standard practice in stratigraphic articles, and whole symposia were organized on the subject (see the Journal of Geology for 1909 and the Geological Society of America Bulletin for 1916; the latter symposium was called "General consideration of paleontologic criteria used in determining time relations," but the word "correlation" appears in the title of each of the four articles). Evidently the word had become common coin, and evidently it connoted time relations.

Powell in 1888 (1888; 1890, p. 63-67) drew a sharp distinction between the formations, as cartographic units, and the geologic time units with which the formations were to be correlated. Under the influence of E. O. Ulrich, one of the most influential stratigraphers of the first third of the 20th century, the author of many correlation charts and the inspirer of many more, this distinction tended however to disappear. Ulrich consistently built up the time-stratigraphic framework of his charts out of formations as building blocks, and he and his followers tended to consider the formations themselves as time-defined, mutually exclusive units. Thus stratigraphic correlation came to include the interrelation of formations, defined by time, with each other as well as with an abstract time sequence.

In the 1920's, the petroleum industry began to demand, and to support, more and more stratigraphic work; the extraordinary expansion of stratigraphy under this stimulus is known to all geologists. In the '30s and '40s, moreover, partly under this stimulus, stratigraphers revived the sharp distinction between, on the one hand, the formation or mapping unit defined on rock type alone (rock-stratigraphic unit) and, on the other, the unit based on time, by whatever means time relations may be determined (time-stratigraphic unit). In the process the term *correlation* has been pulled two ways. One group of geologists wishes to return more nearly to the original usage and restrict the term to time relations; the other group wishes to include under the term all kinds of interrelations of formations and other units—not just those of time. The divergence may be illustrated by two definitions from recent textbooks: "Correlation is the process by which stratigraphers attempt to determine the mutual time relations of local sections" (Dunbar and Rodgers, 1957, p. 271), and "Stratigraphic correlation is the demonstration of equivalency of stratigraphic units . . . in terms of lithologic or biologic continuity or in terms of the geologic time scale" (Krumbein and Sloss, 1951, p. 287). And, carrying the second point of view even further, Shepard Lowman (1949a, p. 147-151; 1949b, p. 1967-1971) has protested vigorously against any use of time-terminology in stratigraphic correlation, which he prefers to express only in terms of "stratal or faunal continuity." I take it this controversy is what I am asked

to discuss, and my known reactionary views on the subject the reason I was asked to discuss it.

THE CRITERIA OF CORRELATION

Before discussing it, however, I would like to consider how we establish correlation, whether that term includes for us stratal or faunal continuity without reference to time or only time relations. There is no need to list all the available criteria or to evaluate them; that is done in the textbooks referred to and elsewhere. Two main classes of criteria are generally recognized: the lithologic and the biologic.

Most of the lithologic or physical criteria are in essence attempts to trace beds, groups of beds, members, or formations, to establish "stratal continuity" whether for its own sake or in the belief that it tells us at least something about time relations. Individual beds of certain special kinds, as for example a bed of volcanic ash, or individual beds with certain special peculiarities, as for example a limestone bed with unique chert nodules in its upper third, serve the best, as far as they are traceable and distinguishable from similar beds above and below (for even the most peculiar bed represents a particular depositional environment that might be repeated). Beds of more banal rock types, however, are not ordinarily distinct enough to be traced and kept separate from the adjacent beds, and it is notorious that the boundaries of members and formations need not remain parallel to either individual beds or assumed planes of contemporaneity, and indeed rarely do, if followed across regional facies trends, though they may exhibit a sort of "stratal continuity" if the intertonguing of facies is not too deep. Finally, the ultimate limit of all these criteria is the limit of traceability of the beds or grosser units—in general, the limit of the individual basin of deposition.

One subclass among the physical criteria stands apart from the others—namely, radioactive age determinations, calculated from measurements of parent and daughter elements or isotopes. No question of continuity is involved here; only the comparison of independently determined, absolute ages, generally rounded off to millions of years. The individual ages so determined are still imprecise, because of analytical difficulties and difficulties in calibrating the different age methods with each other, and still relatively scarce and erratic in distribution, because of the rarity of suitable materials and the time, care, and expense required for first-rate determinations. Radioactive methods therefore, though they provide invaluable information on absolute ages, are of little or no value in correlation—the determination of relative time relations—except where biologic criteria are wanting, as in the Precambrian or in metamorphic terranes, or where the dates can be determined in thousands instead of millions of years, as in the latest Pleistocene.

The other main class comprises the biologic or paleontologic criteria. Now fossils may be used as criteria of correlation in two ways—either simply as intrinsic peculiarities of the rocks that contain them, commonly reflecting faithfully the environments that produced the rocks, or as the expression of an irreversible time sequence, the sequence of life forms produced by organic evolution. Actually, most stratigraphers in most problems use them in both

ways at once. If used in the first way alone, they differ from lithologic criteria only because they are more varied and have more special peculiarities than mineral grains or rock types; what is attempted is the same—the establishment of the continuity and, hopefully, the contemporaneity of beds or groups within individual basins of deposition—and the pitfalls are also the same—mainly facies changes that invalidate the assumed continuity or contemporaneity. Because there may be several different kinds of fossils affected differently by changes in environment, more kinds of criteria are available for detecting the facies changes and establishing the true relations, but the principle is the same as with lithologic criteria.

But once fossils are considered as evidence for the evolution of life through geologic time, a vast extension of their use becomes possible, for that sequence, quite unlike the sequence of rock types[1], is irreversible; fossil forms appear in a determinable order, and once extinct they never reappear. Fossils, and fossils alone, have made it possible to determine the relative ages of rocks on different continents to the degree of precision to which we are now accustomed; in effect, they alone have made other than local stratigraphic correlation possible. The extent of this precision in long-range correlation is quite exactly expressed by our practical time-stratigraphic terminology, which was indeed developed to express it; thus the geologic eras and systems have been recognized throughout the world (though with some difficulty locally in entirely non-marine deposits like those of the Karroo basin of South Africa); series and stages are almost equally world-wide in certain parts of the record where the fossils are particularly well adapted to precise correlation (as in the Jurassic system with its ammonites and the black shale facies of the Ordovician with its graptolites), but are only continental or provincial in other parts; and zones are mainly provincial in extent, with some exceptions.[2]

Although the two uses of fossils can be kept conceptually distinct, they

[1] Fairbridge (1954) has suggested that certain rock types, mainly among the carbonates, may be confined to particular geologic systems though present on several continents, so that they also may be considered to form an irreversible sequence and hence to permit world-wide lithologic correlation. Virtually all these rock types are biogenic, however, and reflect the progress of organic evolution; even if such correlation is possible, the exception is less glaring than at first appears. Radioactive age determinations are another exception, but, as noted above, they permit at present only rather gross "correlation."

[2] I still stubbornly persist in my belief that zone, stage, series, and system are all the same kind of stratigraphic unit, whatever that kind of unit may be called, and in my rejection of the currently popular distinction betwen time-stratigraphic and biostratigraphic categories of units. All agree that zones (in the sense in which the term was first used in stratigraphy by Oppel; nowadays some prefer to call these faunizones, assemblage-zones, or cenozones) are defined by assemblages of fossils. But stages have been defined in precisely the same way ever since the term was first introduced by d'Orbigny, by assemblages of fossils, and likewise beds can be assigned to a given series or system (except in the trivial case of beds near the type locality) only if fossil evidence of some kind can be found, either in the beds in question or in others near enough so that the local stratigraphic relations are reasonably clear. In the absence of fossils, no assignments as precise as system can be made, only such vague ones as pre-Triassic or Precambrian, which themselves merely state that the rocks are older than the oldest fossiliferous rocks in the particular region. If time-stratigraphic units are to be *defined* as units "fundamentally independent of all physical, objective properties of the rocks," then (a) such units have no practical value in stratigraphy, and (b) system, series, and stage, as used every day by stratigraphers, have never been and are not now such units, any more than zone is. For further arguments pro and con, compare Rodgers, 1954, and Hedberg,1954.

are not easily (nor commonly) separated in practice. The stratigrapher studying the fossils of an area uses them both to determine the interrelations of the beds within the area and to correlate those beds with the standard time-stratigraphic sequence, i.e. with beds of the same age on the other continents. The difficulty is that the same fossils may be of different value for the two different tasks; fossils that mark excellent local "key beds" may be very long-ranging elsewhere, whereas fossils that may be significant in determining what series or stage is represented may be strongly influenced by facies and hence deceptive for local use. Most commonly perhaps, the fossils are not perfectly adapted to either use, and different fossil groups give contradictory results because differently influenced by the facies. These situations can become very frustrating, and can lead to the view that, since fossils are fallible criteria of correlation, paleontologic correlation is fallacious. But what the frustration proves is rather that we are using an intrinsically good tool at the limits of its range of usefulness, like a microscope used to look at objects barely larger than the wave length of light. For local correlation, fossils are only one of several tools available, of which now one is more useful, now another; we would be less than intelligent to refuse the help of any of them in solving the difficult problems before us. For general correlation, fossils are much the best tool we have, and we should spend our energies not griping about the tool's deficiencies but trying to sharpen it by more and more careful work on the fossils themselves, with special emphasis on their stratigraphic variations, in short by more monographic studies of fossil faunas.

APPLICATION TO AN EXAMPLE

But let us get back to our subject, the meaning of correlation, and examine it in terms of a particular stratigraphic example. The Cambrian strata of the Grand Canyon (fig. 1) display one of the best documented and most straight-forward cases of facies shifts in the record, a veritable "textbook example"—now that the painstaking work of McKee (1945) has made it clear for us. In any one section three chief rock types occur in sequence—at the base virtually unfossiliferous sandstone, in the middle trilobite-bearing shale, and at the top carbonate rock with sparse fossils in a few beds. These were early distinguished as separate formations and given geographic names; apparently it was tacitly assumed that each was of roughly the same age throughout, for each can be readily traced from one end of the Canyon to the other. McKee's work showed, however, that they record three major facies transgressing eastward one above the other, though with minor regressions at intervals, and that the Bright Angel shale is all or virtually all Lower Cambrian at the west end of the Canyon but all Middle Cambrian at the east end.

What then is the "correlation" of the Bright Angel shale? If we take the term in the broader sense, there are several. Clearly the shale unit is continuous throughout the Canyon, and on this basis the shale at the east end "correlates" with the shale at the west end. If individual beds in the shale could be traced, however (as by walking out key beds or by matching kicks in electric logs in closely spaced wells, assuming key beds or kicks were present), presumably the facies change would be detected, and on this basis

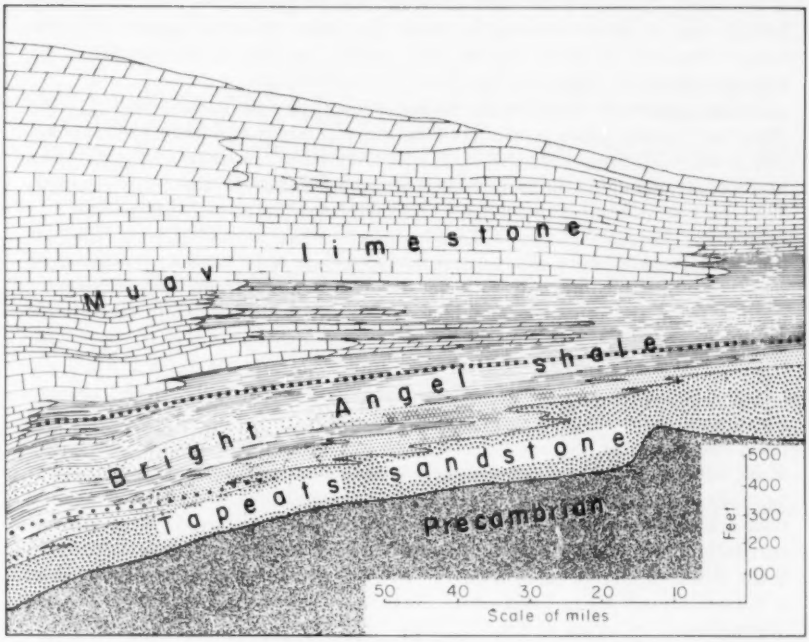

Fig. 1. East-west section of the Cambrian formations exposed in the Grand Canyon of the Colorado in northwestern Arizona. Drawing by C. O. Dunbar after E. D. McKee (1945). Stratification lines indicate isochronous deposits. The row of heavy round dots marks a high Lower Cambrian faunal zone (*Olenellus*); the row of heavy square dots marks a low Middle Cambrian faunal zone (*Glossopleura*). Reprinted with permission from Dunbar and Rodgers, 1957 (fig. 73, p. 141), John Wiley & Sons, Inc.

the shale at the east end would "correlate" with the lower part of the Muav limestone at the west end. Yet both "correlations" are based on "stratal continuity." Similarly, if the fossils were not studied with care, the trilobite fauna in the shale at the east end might be "correlated" by biologic continuity with that in the shale at the west end, but a trilobite specialist would soon discover that new forms invaded the shale during the transgression, and he would prefer to correlate the fauna in the shale at the east end with the sparser fauna in the limestone at the west end. Finally, in terms of time, the older idea was that the shale was contemporaneous throughout, but McKee, by careful attention to the distinction between transgressive and regressive phases and by tracing key beds and fossil zones, showed that correlation to be incorrect and replaced it by another.

Thus in each case two possible correlations are offered, but only time, it seems to me, provides a clear-cut basis for choosing one over the other. If we choose the stratal continuity of individual beds instead of the more obvious stratal continuity of the shale unit, we do so because we think the beds are more likely to be contemporaneous throughout than the formation. Similarly,

if we reject "facies fossils" and correlate instead by the few that cross facies boundaries, we do so because we think the latter are more significant time-wise. Ultimately, in other words, we evaluate our criteria of correlation by their usefulness in demonstrating the true time relations.

But apparently the idea of having more than one "correlation" for the same unit bothers some people less than me. Krumbein and Sloss (1951, chap. 10) distinguish three different kinds of "correlation" in terms of the different kinds of stratigraphic units correlated and the different kinds of criteria used. Thus rock-stratigraphic units are to be correlated "in terms of lithologic . . . continuity" by lithologic criteria, and similarly biostratigraphic units, whereas time-stratigraphic units are to be correlated "in terms of the geologic time scale" by time-stratigraphic criteria listed separately from, and apparently distinct from, the lithologic and biologic criteria, or rather from the criteria for lithologic and biologic correlation. But surely this neat separation is contradicted by every real example of correlation. Working out the true lithologic relations ("stratal continuity") of the rock formations in the Grand Canyon was greatly aided by if not entirely dependent on working out the fossils; on the other hand, the true biologic relations of facies faunas may only be recognized because lithologic criteria suggest the intertonguing of two rock types carrying different fossils. And above all, the establishment of the mutual time relations of stratigraphic units of any kind involves every possible criterion of correlation, not just certain ones; we would be remiss if we ignored any of them, though different criteria may be more useful in different problems. Thus it is perfectly proper to speak of the correlation of two rock-stratigraphic units (formations) in terms of time, and the criteria by which this correlation is established may be a mixture of lithologic, environmental biologic, and evolutionary biologic criteria. And when one speaks of the correlation, i.e. equivalency, of two formations in terms of lithologic continuity, either one is simply saying that they hold the same relative position among shifting facies, or one is suppressing the premiss that they are equivalent because they are contemporaneous.

Accordingly, to no one's surprise, I conclude that in stratigraphy the term *correlation* should and in fact ordinarily does mean the attempt to determine time relationships among strata, however they may be divided into stratigraphic units, and that all the criteria of correlation, imperfect though they may be, are means to this end. It may be more honest, as Lowman has urged, to state our conclusions in terms of the actual criteria rather than in terms of the assumed time relations, but the very reason we accept these criteria and not others is that we hope they will lead us to understand the time relations. Understanding the relationships of continuous units that transgress time is part of understanding time relations, but, once we understand the transgression, to say that two non-contemporaneous parts of such a unit "correlate" only confuses the issue; their relation is that of facies equivalence. Otherwise we will take a word that has had a fairly precise meaning in stratigraphy and turn it into a source of ambiguity, so that we misunderstand each other even more than at present and interpose an unnecessary, dull, and man-

made difficulty to distract us from the really interesting problems, the ones in the rocks themselves.

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[American Journal of Science, Vol. 257, December 1959, P. 692-706]

STRATIGRAPHIC UNITS IN SPACE AND TIME

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ABSTRACT. The physical basis for stratigraphy should be restricted to surface-accumulated (sedimentary and volcanic) rocks; intrusive igneous, metamorphic, and other post-depositionally emplaced bodies should be excluded.

Relationships involve at least a four-dimensional space-time continuum which, for ease of concept and convenience of graphic portrayal, may be subdivided into two three-dimensional frameworks: (1) a spatial framework in which all intrinsically *objective* (physical and biostratigraphic) units are defined; and (2) a framework with lateral spatial dimensions and a vertical temporal dimension, in which all *subjective* (time-stratigraphic) units are defined.

Transposition (with appropriate distortion) of the objective, spatial elements to the subjective, space-time framework (or *vice versa*) provides bases for integration and potentially complete historical interpretation. Moreover, such an integrated system, insofar as it may be dimensionally valid and otherwise logically constructed, tends to induce consideration of various essential relationships which commonly have been disregarded in stratigraphic analyses and historical interpretations.

Present geologic knowledge and practices imply that four basic principles and numerous distinctional factors or criteria must be satisfied. The principles are superposition, faunal succession, base-level, and datum variance. Among the distinctional criteria are subjectivity and objectivity; physical, bio-, and time-stratigraphic; environmental ("eco-stratigraphic") and non-environmetal control; natural and arbitrary configuration; mode of contiguity (vertical, lateral, and intertongued); mappability and scale; lithology; unconformity; evolution; "uniformity" and heterogeneity; barrenness; lateral variation (facies); deposition, non-deposition, and erosion (stratigraphic cycle); tangibility; and continuity versus discontinuity.

Although the number of factors appears unwieldly, if the physical, bio- and time-stratigraphic categories are shown on one co-ordinate axis, and if the other factors, as additional bases for unit configuration, are appropriately applied to the other, a reasonably simple, dual (space and space-time) classification may be constructed.

INTRODUCTION

A principal objective of many stratigraphers has been to keep concepts and terminology "simple and practical." Indeed, these would be worthy aims, if the science could advance within the simplicity of a framework largely developed a century ago, and if the tendency to justify the *status quo* by fettishing the word "practical" does not actually lead to malpractice.

Hedberg (1958) has recently expressed this desire for simplicity, although he properly included *verity* and *utility* as other essential attributes. He pointed out that: "Stratigraphic terminology, like stratigraphic classification is . . . . only a means to an end [but that] it plays a very important role in stratigraphic thinking."

To realize the aims of verity and utility, however, terminology must reflect the validity and adequacy of stratigraphic concepts and procedures, in the interest of creative thinking and communication. From this viewpoint an attempt is made to bring together a space-time framework, incorporating essential principles, concepts, procedures, and terminology. Because the "mental crutch" of classification is involved (fig. 1), the approach follows the devious course of numerous factors or kinds of criteria and their influence on the designation of units.

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reasonable facility, but which nevertheless defies imagination. If this disquieting thought has created a dilemma, it is one which can no longer be ignored. We must either adapt some kind of "non-Euclidean" geometry in order to portray the complex, or we must employ a device within the realm of familiarity which will permit us to deal in the customary three dimensions. Because the former does not appear to lie within the scope of the profession's (and the writer's) present capabilities, it is assumed that the latter is more feasible. This appears to be attainable by: (1), defining all physical and biostratigraphic (objective) entities in a three-dimensional spatial framework; and (2), defining all time-stratigraphic (subjective) entities in a framework consisting of two lateral spatial dimensions and a vertical dimension in time.

Such a dual system is not as simple as the single three-dimensional spatial scheme into which, with persistent futility, we have attempted to cram the troublesome fourth dimension. Williams (1894) and others since have observed the need for such a dual system, but elements of the two sub-systems have been confused, and their dimensional characteristics generally have not been envisaged. Most of the space-time relationships were illustrated by Blackwelder (1909), but his significant concepts have not received their deserved consideration. Although Williams' observation was injected into the classification by Schenck and Muller (1941), most of Blackwelder's implications have escaped proper notice for a half-century.

Why has an integrated four-dimensional system continued to remain elusive? In line with the tendency to maintain "simplicity and practicality" of concept and terminology, all geologists have "learned" that there are only two fundamental laws in stratigraphy, superposition and faunal succession (and it may be argued that these two are merely differing expressions of a single law relating to vertical succession). If these were the only laws involved, stratigraphy would indeed be simple—so simple, in fact, that the entire stratal "sphere" would consist of a series of concentric layers. The varied relationships, however, can only be explained in terms of at least two additional principles (baselevel and datum variance), the implications of which demand a more complex system.

Stratigraphy is variously defined, but among recent authors, Dunbar and Rodgers (1957) refer to the subject as:

> . . . the study of stratified rocks . . . . originally spread as sheets over a surface of accumulation. [It includes] the study of such layered igneous rocks as ash falls and lava flows, and of metamorphic rocks insofar as they reflect the original sedimentary [or volcanic] character—a definition in agreement with the statement of the U.S.S.R. Interdepartmental Stratigraphic Committee (Rotay, et al., 1956).

Stratigraphic units should *directly* relate to surface-accumulated rocks; and if stratigraphy is to have any restriction in the vast realm of geology, entities more directly related to other geological phenomena should be excluded.

The American Commission on Stratigraphic Nomenclature (1956, p. 2000-2006) cited the time-honored and useful rock classes: sedimentary, igneous, and metamorphic; and implied that all units of each are rock-stratigraphic. Such an all-encompassing view not only needlessly flouts the oldest and most fundamental law (superposition), but it also confuses stratigraphy with petrology, and gives rise to illogical practices. The distinction between surface-

deposited (stratigraphic) layers and postdepositionally emplaced rocks is fundamental; post-depositionally emplaced bodies, regardless of whether they may be petrologically classed as igneous, metamorphic, or "sedimentary" (sandstone dikes and salt intrusions), should not be regarded as stratigraphic units. The Dunbar and Rodgers and Russian definitions carry the inescapable implication that the law of superposition must obtain. It is believed that this simple test serves to distinguish stratigraphic from non-stratigraphic units.

Wheeler and Beesley (1948) referred to stratigraphy as being four-dimensional. Stainforth (1958) stated: "Accepting as we must, that sedimentary [and volcanic] rocks form a four-dimensional continuum, this very word tells us why stratigraphic classification and nomenclature remain in a debatable state." Schenck and Muller (1941) opened the door to this four-dimensional concept, not only by pointing out the distinction between "time" and lithic units, but also by introducing a time-stratigraphic category. However they confused the dimensional aspects of the problem, as did Wheeler and Beesley (1948), by assuming identity of biostratigraphic and time surfaces. Hedberg (1948) appears to be the first to emphatically disparage the traditional belief that biostratigraphy is a wholly reliable basis for determining synchrony. In a paper based largely on this view, Wheeler, et al. (1950) made an abortive attempt to reconcile the various concepts by proposing the term "para-time-rock" for any biostratigraphic or lithologic units that are deemed to "approach synchrony." The American Commission on Stratigraphic Nomenclature (1952, p. 1628) disregarded this ill-considered proposal, and gave long-overdue recognition to the distinction of the biostratigraphic and temporal divisions. The Commission thus recognized four principal kinds of units—rock-stratigraphic, biostratigraphic, time-stratigraphic, and "geologic time" units. Three of these four have been accepted and discussed as primary categories in papers on lithostratigraphy (Wheeler and Mallory, 1956), on biostratigraphy (Wheeler, 1958a), and on time stratigraphy (Wheeler, 1958b).

Each of these three was approached with emphasis on the dimensional relationships. Both physical and biostratigraphic units are mutual but distinctive occupants of the same three-dimensional spatial framework and are designed to account for total stratigraphic space. It logically follows that a category must be designed to potentially account for total stratigraphic space-time. But if it is not feasable to deal singly with the four-dimensional continuum, the conclusion seems inescapable that here also a three-dimensional framework must be employed. In time-stratigraphy, the same geographic points are involved. Thus the two lateral dimensions are common to all phases of stratigraphy. This leaves the vertical as the time dimension of the three dimensional space-time framework.

From this point of view it may be seen that there is neither a place nor a need for "geologic time" units in the non-stratigraphic sense proposed by the American Commission on Stratigraphic Nomenclature (1952, p. 1628-1629). The notion of "non-stratigraphic time" in stratigraphy is as dimensionally invalid and useless as would be the concept of a "purely biologic ("non-stratigraphic") zone. Time has no meaning in stratigraphy unless it is first tied to the spatial record and then substituted for its vertical dimension. Thus

the units involved (period, epoch, and age) are both three-dimensional and time-stratigraphic (of which time is but a single dimension). Like all other time-stratigraphic units, they have a vertical dimension measurable in terms of time; and like all stratigraphic units of any kind, their lateral dimensions are spatial. Period, epoch and age differ from other time stratigraphic units only in their constant time values and their potential world-wide continuity.

Most of this discussion up to this point, either directly or indirectly, involves dimensional relationships. In this regard, it is noteworthy that the general disregard for principles other than superposition-faunal succession has given rise to terminology which not only futilely attempts to substitute three dimensions for the necessary four, but also which commonly reflects thinking in terms of only a single dimension.

KINDS OF STRATIGRAPHY CRITERIA

Physical Stratigraphy

As indicated in the foregoing discussion, the Stratigraphic Commission and others, in recent years, have recognized lithostratigraphic, biostratigraphic, and time-stratigraphic as three fundamentally differing kinds of first-order categories. Units of the "lithostratigraphic" category indeed consist of rock, but some of the criteria by which units are distinguished are other than lithologic. For this reason, the term physical stratigraphic seems more appropriate for this first category. Among the more important criteria used in the designation of physical units are: lithologic character (lithosome and lithofacies); unconformities (sequence); and mappability, including lithology and physiographic expression (group, formation and member).

Biostratigraphy

If individual fossils are viewed as representatives of their respective taxa, they occur disseminated through the rocks in a manner which delineates the stratigraphic spatial distribution of that taxon. Such three-dimensional distribution patterns may be viewed either by themselves, or with regard to the dimensional configuration and contiguity of their various associations in stratigraphic space. The resulting units are truly biostratigraphic, for they are based on fossils as biologic rather than lithologic criteria.

Time-Stratigraphy

An appreciable segment of the geological profession has implied in recent years and, in some cases, continues to hold that "fossils at present provide the only evidence for the placing of rocks in a world-wide geologic time scale" (Teichert, 1957, p. 2575). This notion has been challenged by Hedberg (1948 and 1951), Wheeler, et al. (1950), The American Commission on Stratigraphic Nomenclature (1952), and Woodring (1953), among others, but appears to remain one of the most strongly contested major issues in the entire "classification controversy."

Another review of the evidence is not justified here. It should suffice to restate that all *objective* datum surfaces are intrinsically variable in their relation to stratigraphic time surfaces. This does not belittle the value of biostratigraphy as a basis for temporal correlation. Instead, it merely implies that

this most widely employed approach to dating should be augmented by other objective criteria, and above all, that biostratigraphic and physical criteria should be fully integrated.

Objective and Subjective Criteria

The American Stratigraphic Commission (1957, fig. 1) indicated that all stratigraphic units may be placed into one or the other of two categories on the basis of their essential objectivity or subjectivity. The Commission pointed out that biostratigraphic units, together with lithostratigraphic, and other physically based units, are "dominantly objective units [in the sense that they are] based largely on observation and measurement [and that chronostratigraphic units belong in a category of] dominantly subjective units based largely on interpretation." This conclusion is obviated by the subjective nature of the temporal dimension in a space-time framework.

Hedberg (1958, p. 1883-85) has made a special point of this distinction.

Environment

The spatial distribution of the various specific types of sedimentary and volcanic rocks is entirely controlled by the space-time distribution of *environmental* factors. The same may be said for some kinds of biostratigraphic entities, the spatial distribution of which is primarily influenced by the ever-changing distribution of ecologies (Moore, 1957).

Whether environment influences configuration of some time-stratigraphic units depends on one's definition of time-stratigraphy. If time-stratigraphic units include all three-dimensional entities related to the stratal record, and defined in a framework consisting of two lateral space dimensions and a vertical temporal dimension, such units, because they are designed to account for total space-time must include, not only the preserved stratal record in space-time, but also such entities as interpreted deposition and non-deposition. Obviously, the configuration of such entities is the consequence of space-time migration of depositional, non-depositional, and erosional environments.

"Natural" and Arbitrary Limitation

The notion is common that most stratigraphic units denote the more significant events of depositional history; that is, that the boundaries between units mark the more important responses to change through geologic time. This is true in some cases; and it would be desirable that it were true in every case. Some of the most widely employed units, however, are primarily designed to fulfill special needs, with the result that their boundaries are arbitrarily defined in such fashion that the natural configurations are obscured.

Sequences are defined by their bounding unconformities, and lithostromes depict the distribution of "uniform" lithologies. Configurations of such units are largely natural. Formations, however, are defined for their mappability and may be decidedly arbitrary.

Similarly, a given biostratigraphic zonation represents only one of several possible arbitrary subdivisions based on taxon ranges; while zonules and biosomes based on "uniformity" of assemblage are "natural" in configuration.

In time-stratigraphy, boundaries between units of constant time value

must be established arbitrarily; and those between time-variable units occurring within maximum time limits (such as system, series, and stage) are in part arbitrary and in part natural. Configuration of all other space-time units is controlled by natural phenomena such as deposition, non-deposition and erosion.

Contiguity

Stratigraphers have been reluctant to conceive and accept dimensional relationships other than those involving vertical succession. Nevertheless, intertonguing of certain lithic, paleobiologic, or time-stratigraphic units is the inevitable consequence of datum variance; and the baselevel concept (Barrell, 1917) cannot be divorced from units based on interpreted space-time distribution of deposition and nondeposition. In addition, designation of laterally contiguous facies units has been demonstrated to have special utility in certain phases of stratigraphic analysis.

Configuration of all stratal entities relates directly to the modes of contiguity; that is, whether the units are vertically, vertico-laterally, or laterally contiguous. Distinction among various units in each of the three categories (physical, biostratigraphic, and geochronologic) depends on recognition of these dimensional relationships.

Mappability, Scale, and other "Purpose" Considerations

Cohee (American Comm. on Stratigr. Nomen., 1956, p. 2014) and associates have stated:

> Since the early days of geologic mapping in this country the need of stratigraphic units for cartographic purposes has been recognized, and these fundamental units have been considered formations. . . . . it is recommended that the concept of mappability be considered an essential feature in the definition of a formation.

We should endorse these views on mappability, not only with regard to formation, but also group and member. This endorsement should not imply, however, that other stratigraphic units which are amenable to map portrayal should not be mapped.

One aspect of scale affects configuration; that is, recognition or nonrecognition of stratal layers. Wheeler and Mallory (1956, fig. 4) illustrated the deliberate use of the arbitrary cut-off as a necessary expedient involving arbitrary assignment of certain intertongued layers in the designation of formations. They implied that all such intertongued lithostromal bodies are individually recognized and assigned to their respective lithosomes. In many cases, however, if this procedure is carried to the extreme of recognizing each thin bed and lamination, the consequent configurations would be both impossible to achieve and undesirable as workable units. In other words, as an effect of scale, many thin layers that are recognized in concept must be individually disregarded in practice. This recognition factor may also apply in biostratigraphy and time-stratigraphy.

Lithology

Lithology is used in four differing senses in its influence on the delineation of those physical units to which it applies.

Specific lithologies are of primary and direct concern in defining such units as lithostrome, lithsome, group, formation and member; whereas *lithologic aspect* is derived statistically as the basis for the definition of laterally variant (lithofacies) units. Such aspect, of course, depends on the number, kinds, and relative proportions of specific lithologies present.

Among the above-mentioned lithic units, the configuration of lithostrome and lithosome is controlled solely by their lithologic "uniformity", while group, formation, and member may be *variant* in that they may comprise arbitrary assemblages of various specific lithologies. The fact that some formations and members may be lithologically uniform is coincidental, for such uniformity is not among their inherent characteristics.

The term *uniform*, as used here requires definition. In the case of some units, the uniformity is characterized by relative purity of both composition and texture; whereas others possess uniformity by virtue of their lithologic heterogeneity. A unit comprising thin intercalations of limestone and siltstone may be regarded as uniform, but only by the afore-mentioned non-recognition (as units) of its differing components. This kind of uniformity could not exist, except for the scale-factor. For example, a body consisting of a succession of glacially varved clays may be designated as a mappable unit of uniform lithology, and may be so employed at most map scales. However, it may not be regarded as uniform in the large-scale view of a glaciologist who locally is concerned with the detail of its individual laminations.

Unconformities

The significance of unconformities was appreciated by most of the founders of stratigraphy to the extent that most of the systems were originally defined by their unconformable relationships. The catastrophist concept and its outgrowth the diastrophist concept, in turn were the products of this early recognition of the role of unconformities in historical interpretation. In recent years, however, Wheeler (1947), Wheeler and Beasley (1948), Gilluly (1949), Spieker (1956), and others have disparaged (with a considerable measure of success) the long-standing notions that unconformities tend to be world-wide, and that, as manifestations of diastrophism, they constitute "the fundamental basis for subdivision of the geological record". Although the unconformity has been losing its traditional role in time-stratigraphic classification, it was not immediately given its deserved consideration for another.

Sloss, Krumbein and Dapples (1949) defined the term *sequence*, and designated several examples, all of which, in the regions under consideration, are bounded by unconformities. Gignoux (1955), in part, employed the term *stage* in this same sense. The American Commission on Stratigraphic Nomenclature (1957, p. 1877) included unconformities among its list of bases for classification. Moreover, many of the units shown on the Geological Map of the U.S.S.R. (Nalivkin, 1956), are of this kind.

These unconformity-based units are necessary, not only because of their direct value in physical stratigraphy, but also because they are essential, as a framework of reference, to the concept and derivation of a number of different varieties of time-stratigraphic units (Wheeler, 1958b).

Biologic Factors

Evolution.—The importance of evolution as one of the bases for subdivision and classification is appreciated by all geologists. Agreement is general that this continuous and irreversible process should be employed as a factor in the control of configuration and contiguity of certain kinds of units. However, a significant point of disagreement concerns the fundamental nature of units so defined. In accord with The American Commission on Stratigraphic Nomenclature (1952 and 1957), the writer agrees that these are biostratigraphic units, and that the criteria involved may not serve directly to delineate time-stratigraphic units, as recently advocated by Jeletzky (1956), for example.

Faunal "Uniformity".—In general, the same kinds of problems exist regarding faunal uniformity as are encountered in lithologic uniformity, except for the additional factor of evolution. However, as the writer has previously indicated, (Wheeler, 1958a), the evolution factor may be minimized by the proper selection of units (zonules). In other words, in spite of evolution, as long as the environment remains constant in a given depositional situation, the plexus or organisms will tend to remain relatively constant. "Uniformity" of assemblage, as of lithology, must be permitted to imply either relative purity or heterogeneity, as the case demands.

"Barrenness".—Because fossils are unknown in some segments of the stratigraphic record, or because of the absence of fossils of the kinds that are otherwise employed in a given biostratigraphic subdivision, the American Commission on Stratigraphic Nomenclature (1957, p. 1880), has recommended use of the term *barren zone* for such faunally negative units. The writer, however, has pointed out that evolution is the primary control factor in zonation and that barrenness is the consequence of environmental influences (Wheeler, 1958a, p. 651). From this it may be concluded that only zonules and biosomes may be barren, and that the term barren zone is therefore inappropriate.

Biologic Aspect.—In the foregoing discussion of lithology as a factor in configuration, laterally variant lithologic aspect units (lithofacies) are mentioned. Similarly, ratios involving two or more laterally intergrading paleobiologic associations may serve to delineate biofacies. The writer's previous discussion of biofacies units (Wheeler, 1958a, p. 650) does not need repeating here. The present purpose is merely to point out the role of biologic aspect as another of the many factors in stratigraphic unit configuration.

Temporal Limitation

Arbitrary Temporal Limitation.—It has become evident in recent years, that the boundaries between units of constant time value ultimately must be established arbitrarily within continuous depositional successions. These arbitrarily defined time-datum surfaces are the boundaries between such space-time units as period, epoch, and age. Hence, these units may be regarded as universally present, arbitrary slices of total stratigraphic space-time.

Variable Temporal Limitation.—The above mentioned units of total space-time comprise a space-time framework in which "bodies" comprising the preserved stratigraphic record, interpreted erosional removal, and interpreted

of such subjectively delineated time-stratigraphic "bodies" are obviously time-variable.

Stratigraphic Cycle and Baselevel Concept

It is a generally neglected fundamental that geologic history at any given place comprises an alternating succession of depositional and non-depositional episodes. From this viewpoint, any properly constructed, time-stratigraphic column is cyclic.

The stratigraphic cycle has been defined as comprising "the entire space-time 'volume' represented by both the surface accumulation (holostrome) and non-depositional (hiatus) interpreted for the interval separating two successive unconformities" (Wheeler, 1958b). Its tangible manifestation, therefore, is in every case a sequence in the restricted sense. Implied in this definition, however, is more than mere deposition followed by nondeposition, for all such cycles involve, during the nondepositional phase, at least some erosional removal of strata belonging to the depositional phase; and in many cases all tangible evidence of the cycle may be locally, and sometimes regionally, erased. Such removal, nevertheless, does not imply absence of the cycle, because complete interpretation of geologic history anywhere must involve, among other things, the interpretation of the entire succession of stratigraphic cycles, regardless of whether they may be locally represented in the present record.

These are not the "sedimentary cycles" envisaged by Gignoux (1955, p. 18). As his term indicates, Gignoux's cycle is only sedimentary. It is, essentially, a specific variety of sequence in the restricted sense. The stratigraphic cycle, however, is more than sedimentary in that it is time-stratigraphic in nature; it includes the post-depositional lacuna as well as the sequence (it is thus partially subjective); and it may embody marine sediments, non-marine sediments, volcanic rocks, or their various combinations. In other words it is a complete time-stratigraphic cycle extending at any given place from the initiation of accumulation, through its cessation, and through the subsequent phase of non-deposition, to the place-time beginning of the depositional phase of the next succeeding cycle. Fully interpreted geologic history is possible only where the complete succession of such cycles can be envisaged. These cycles and their phases, in turn, can only be fully appreciated in terms of the baselevel concept.

Barrell (1917, p. 778) stated:

> . . . . the sediments [and volcanics] whose interpretation form the basis of earth history have been characteristically deposited with respect to a nearly horizontal controlling surface. This surface of control is baselevel, but for continental and marine [and volcanic] deposits the baselevel is determined by different agencies and is a word of more inclusive content than the sense in which it has generally been used by physiographers as a level limiting the depth of fluviatile erosion. Sedimentation as well as erosion is controlled by baselevel, and baselevel, local or regional, is that surface toward which the external forces strive, the surface at which neither erosion nor sedimentation takes place.

This definition, which the writer believes to be both valid and fundamental, has been generally misinterpreted. In contrast with popular concept, baselevel is neither a "horizontal plane" nor can it be defined solely in terms of sea-level or relationships on the sea floor. At any given moment its intersection with the surface of the lithosphere on one profile may be, say, a thou-

non-accumulation. These may be subjectively allocated in the interest of complete historical interpretation. The boundaries (and hence the configurations) sand feet above sea-level and a thousand miles inland; while on another it may be a fifty fathoms below sea-level and fifty miles off-shore. On some profiles baselevel may alternately rise and fall to intersect the lithosphere surface at a number of different points. Moreover, baselevel should not be conceived solely in its relationships to either erosion or aggradation alone, for its significance is best appreciated in stratigraphy, sedimentation, or geomorphology if it is conceived as a "surface at which neither erosion nor sedimentation [can take] place". Actually, the position of any point on the surface at any given time, relative to baselevel, is controlled by the increment and transport-energy relationship. It makes no difference whether the increment is organically or otherwise chemically generated, whether it is erosionally derived, or whether it is a direct product of volcanic activity. Nor does it matter whether the transport medium is water, ice, or atmosphere. The position of a given surface point, relative to baselevel, depends simply on whether the increment exceeds, equals, or is less than that which the energy can move. From this viewpoint, baselevel, like other geological conditions, is in a continual state of space-time flux.

Baselevel and the related grade concept are of fundamental importance in time-stratigraphy and therefore in stratigraphic analysis and historical interpretation. The writer has shown previously that insofar as they may be accurately delineated, the continuous space-time shift of the points of grade is marked by the boundaries between depositional and hiatal units (Wheeler, 1958b, p. 1061). The baselevel concept and the related concept of the stratigraphic cycle are required for an understanding of the derivation and configuration of most time-stratigraphic units, to say nothing of their influence in physical and biostratigraphy. Some time-stratigraphic units, for example, are invariably confined to the space-time "volume" of a single stratigraphic cycle; some are in all cases multicyclic; one may be either uni- or multicyclic; some may be "fragmented" or separated into disconnected parts by cyclic consequences; and some units are non-cyclic.

Tangibility

The American Commission on Stratigraphic Nomenclature (1952, p. 1629) stated that time-stratigraphic units are material units and that "geologic time units" are non-material and non-stratigraphic. Wheeler (1958b, p. 1059), in disagreement, has argued that the only kind of time which is of use to the stratigrapher must have stratigraphic control, that all units involving a dimension in time are time-stratigraphic, and that the content of such space-time units may be entirely material, entirely non-material, or partly material and partly non-material, depending on their definitions. Again, it is assumed that the aim in time-stratigraphy is to potentially account for total stratigraphic space-time, just as it is in both physical and biostratigraphy to account for all stratigraphic space. If so, preserved depositional entities defined by their positions in the space-time framework are material units; entities interpreted either as the space-time value of non-deposition, or as former deposits later removed

by erosion, are non-material units; and entities such as either "restored" deposition or those with constant time value may be in part material and in part non-material. Hence, tangibility is not only a primary factor in determining the space-time configuration of preserved tangible bodies, but is also a factor without which the shape of non-material units could not be envisaged.

Deposition, Non-deposition, and Erosion

All material stratigraphic units, of course, occur in consequence of deposition, whether they be physical, biostratigraphic, or geochronologic. Furthermore, the present configuration of units in each of the three categories may be in part controlled by post-depositional erosion. Only in time-stratigraphy, however, are non-deposition and erosional removal represented "volumetrically." Satisfactory historical interpretation is not possible without envisaging units the estimated space-time configuration of which is controlled by preserved deposition, original deposition, non-deposition, erosional removal, deposition plus non-deposition, and non-deposition plus ersional removal.

Lateral Continuity and Discontinuity

Only one category of stratigraphic units has potential world-wide continuity. Eon, era, period, epoch, and age may be envisaged as being universally present in the space-time stratigraphic sphere. The Jurassic Period, for example, is present everywhere with precisely the same vertical (time) value and successional position.

All other kinds of stratigraphic units, however, may be laterally discontinuous. Such lateral discontinuity is of two kinds—natural limitation by "pinch-out" (or "cut-out") and arbitrary limitation by the arbitrary cut-off. In physical stratigraphy, both formations and lithostromes, for example, may be terminated by either pinch-out or arbitrary cut-off (Wheeler and Mallory, 1956); and sequences may be terminated arbitrarily or they may be "pinched out (actually cut out) wherever the upper bounding unconformity truncates the lower one (Wheeler, 1958b, fig. 2). In biostratigraphy, zonules may be terminated in either manner, and zones may discontinue laterally by the cut-off (Wheeler, 1958a, fig. 2). Moreover, one may readily envisage that zones are also subject to termination by variance of its boundary surfaces, or by erosional truncation. These same conditions have been shown to apply in the case of vertically successive time-stratigraphic units, with the above-mentioned exception (Wheeler, 1958b). Finally, all somal units, by their very nature, are subject to multiple "pinch-out"; and all facies units are terminated laterally by arbitrary cut-off.

Thus the lateral continuity of eon, era, period, etc., and the discontinuity or intermittency of all other stratigraphic units are factors in their distinction and dimensional configuration.

Datum-Surface Variance

Geologic literature gives many examples of angular relation between rock unit boundaries and "time horizons." Because Wheeler and Beesley (1948, p. 82-84) concluded that this relationship is intrinsically variable in all cases, they called it the "principle of temporal transgression." Presence of the word

temporal appeared to distinguish this connotation of transgression from that of Grabau (1907) and others, who have used the words *transgression, transgress, transgressive,* and *transgressional* to imply marine advance as opposed to *regression* or marine retreat.

This notion of *temporal transgression* was expressed at a stage of stratigraphic thinking when only rocks and time appeared to be involved—a stage characterized by assumed parallelism of time- and biostratigraphic datum surfaces, and a few months before Hedberg's (1948, p. 456) observation that: "The time value of stratigraphic units based on fossils will fluctuate from place to place in much the same manner as the time value of a lithologic formation may vary." More recently, the American Commission on Stratigraphic Nomenclature (1957, p. 1878) recognized this variation by noting that biostratigraphic units may be "roughly parallel with rock-stratigraphic and time-stratigraphic units, but they do not necessarily conform to such units nor are they necessarily restricted by the boundaries of such units." Thus it has become evident that this intrinsic variance applies to any two stratigraphic datum surfaces of any kind or kinds (except among time-stratigraphic surfaces of constant time value when viewed in the subjective, space-time framework). Thus, it is also evident that because this transgression involves more than mere time differential, the word "temporal" is inappropriate. Without this modifier, however, use of the term transgression in this sense would be an obvious source of confusion. Therefore, the *principle of datum variance* is proposed in terminological substitution.

Datum variance is of major significance among the causes of dimensional configuration. Except for this factor, the Wernerian concept of universal concentric lithostratigraphic spheres would obtain, and all stratigraphy would be of the "layer-cake" variety. It is for this reason alone that the principle of datum variance must be included with the baselevel concept, superposition, and faunal succession to provide the foundation for a sound stratigraphic system.

CONCLUSION

It is intended that the foregoing brief discussion of the nature of stratigraphy and some of its more important components may serve to demonstrate the immensity and dimensional complexity of the subject, even though, as recommended, its tangible manifestations may be restricted to the surface-accumulated geologic record.

If it is true that acceptable stratigraphic intelligence and practice must incorporate at least four basic principles rather than the generally recognized two, if the numerous and variable factors employed to date as bases for envisaging and designating stratigraphic units are to continue in use or perhaps expand to greater utility, and if the consequent classification and terminology are to possess the desired qualities of verity and utility, they cannot at the same time be characterized by the degree of simplicity that a segment of the profession currently appears to be demanding. In essence, it is the writer's conviction that the inducement of creative thinking and the conveyance of ideas, in any field of human endeavor, are greatly enhanced if terminology and nomenclature or other symbolism is permitted to keep pace (or catch up)

with development of concepts and techniques. In numerous cases stratigraphers have unsuccessfully attempted to reconcile the increasing need for a more precise language, on the one hand, and the ever-present clamor for simplicity, on the other. This inevitably gives rise to the "Humpty Dumpty" approach cited by Hedberg (1958, p. 1883) by which "a word . . . . means just what I choose it to mean." Fortunately, stratigraphic thinking and practice have advanced to the point where a few basic terms will no longer suffice for the present, not to mention the need for better expression in the interest of future progress.

The fact must be made clear, however, that the writer holds no serious brief for any of the ordination accepted or suggested in these pages, as terms embodying a greater degree of validity and utility may be proposed—or better, if a system possessing the necessary attributes can be significantly simplified. The most that presently can be claimed for this classification is that it comprises a preliminary attempt to construct a dimensionally sound, skeletal, unified, stratigraphic system, in the development of which an effort has been made to incorporate as much as possible of existing notions and terminology. And in the last analysis, if this attempt or any small part of it may be used as an effective springboard for more significant developments in this complicated field, it will have served its intended purpose.

Although the publication space available for this report does not allow inclusion of their verbal definitions, the implied stratigraphic units, together with the principle bases for their distinction, are shown in the accompanying classification (fig. 1).

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[American Journal of Science, Vol. 257, December 1959, P. 707-721]

STRATIGRAPHY—TRADITIONAL AND MODERN CONCEPTS

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ABSTRACT. Traditional stratigraphic units consist of one or more contemporaneous lithologic and faunal facies within a specific layer of rock. Each layer is local and regional in extent, and is separated from other layers by real time-significant boundaries which are either planes of contemporaneity or planes of unconformity. Where the boundaries are indistinct, faunas characteristic of the specific layer, and the stratigraphic succession in general, are used to identify the stratigraphic unit. Because of its regional attributes the stratigraphic unit serves as a correlation unit, as well as being the basic unit for local and regional stratigraphic subdivision and terminology. Zone, stage, series and system are such units which informally may be termed *stratigraphic formations* having regional time-significance.

Accusation by supporters of the American Stratigraphic Commission that geologists fail "to exclude concepts of time from consideration of the objective data on which alone properly defined rock units are differentiated", is not applicable to concepts of traditional stratigraphy. This accusation applies only to modern concepts of Litho-, Bio-, and Chrono-stratigraphy, which, while erroneously considered to be stratigraphic, more correctly are regarded as pre-stratigraphic procedures and concepts. Modern stratigraphic terms do not correspond with traditional concepts because Litho- and Bio-stratigraphy do not discriminate between distinct stratal superposition and different transgressive lithologic facies, or between stratigraphic paleontology and facies fossils. This failure has led to misconceptions in Chrono-stratigraphy and to development of unrealistic and inadequate terms, as well as to misapplication of traditional terms. Consequently modern concepts and terms are arbitrary and of questionable utility except in pre-stratigraphic procedures.

INTRODUCTION

Theoretical descriptions and general terminology for explaining everyday stratigraphic procedures and criteria fail to correspond with the practical and often reasonable results achieved in stratigraphic studies. This is because the practicing stratigrapher is unfamiliar either with the simple, basic language of stratigraphy or with the real distinction that exists between time-significant criteria and facies criteria. As a result modern stratigraphers have broadened specific meanings and restricted very general meanings of concepts and terms to such an extent that it is impossible to communicate satisfactorily on stratigraphic matters.

Although modern and traditional terms are alike, their intended meanings often are quite unlike. This is the reason why traditional and modern concepts appear incompatible.

It is chiefly because of this confusion, that the co-authors of this paper undertook a penetrating study of traditional and modern concepts. The critical reviews and comments of this paper are the result of sincere judgments which, it is hoped, will be taken as sincerely as they are given.

ACKNOWLEDGMENT

The writers acknowledge the assistance of the Husky and Amerada Companies in typing and drafting. Various co-workers and other geologists have been helpful in discussion and editorial suggestions. This paper has benefited particularly from critical discussions with G. O. Raasch, chairman of the

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TRADITION IN STRATIGRAPHY

Goethe has said, "That which our heritage has lent us has to be earned anew in order to possess it". In our case, the stratigraphic heritage is founded on the studies of William Smith. That Smith discovered the meaning of stratigraphy should not, however, be accepted *ipso facto*. Tradition does not necessarily justify a definition of stratigraphy any more than does utility. It is the common sense and the reality on which this tradition itself is based that makes Smith's heritage so valuable to us.

"Strata" Smith, as he was appropriately known to his acquaintances, observed in area after area that an orderly superposition of strata prevailed. He further noted that a particular interval of rock could be readily recognized as distinct from those above and below on the basis of the contained fossils even where this interval was of different lithological composition laterally. Although Smith was the first to demonstrate clearly these features, his real genius lay in their application as criteria of the otherwise not easily demonstrable lateral continuity or equivalency of strata, that is, correlation. This has been excellently and simply stated by Lowman (1949, p. 151): " . . . . stratigraphic correlation can be stated in terms of stratigraphic fact—namely, stratal or faunal continuity". In following this procedure Smith was able to show in his table of 1799 the absence of certain Jurassic beds below the sub-Cretaceous unconformity because of his knowledge of their existence elsewhere (Arkell, 1933, p. 5-6). Thus, from a brief review of Smith's contribution to stratigraphy we can conclude without ambiguity that the original intention of the meaning of the term stratigraphy coincides with its etymology (strati-*strata* or rock-layers; graphy-*graphos*, something written or drawn). It was clear to Smith that strata are objective observable time-significant features which continue universally irrespective of lateral lithologic changes within them (intrastratal) except where they are absent through erosion or non-deposition (fig. 1).

The criteria and principles of stratigraphy so firmly demonstrated by William Smith were primarily responsible for the great surge in stratigraphic research of the 19th century. A further step was the logical development of a nomenclature which served the three purposes of terminology, classification and correlation. In this field d'Orbigny was pre-eminent. Significantly, he continued to follow closely in the tradition of Smith. An exception to this tradition was his rather limited use of lithology as a criterion for correlation, which to some extent, is understandable because of the excellence of the fossil record. His philosophy is expressed in a translation of a sentence from his work of 1852 (Muller and Schenck, 1943, p. 267).

> We have followed these horizons without having regard for the mineralogic composition and we have seen all the difficulties disappear by application of the two criteria of stratigraphic demarcation of stages: superposition and the limits of the faunas in the strata.

D'Orbigny's basic units of stratigraphy were the stage and the zone. His stages, based on local stratigraphy and named after a geographic locality, were utilized for the three purposes of terminology, classification and correlation.

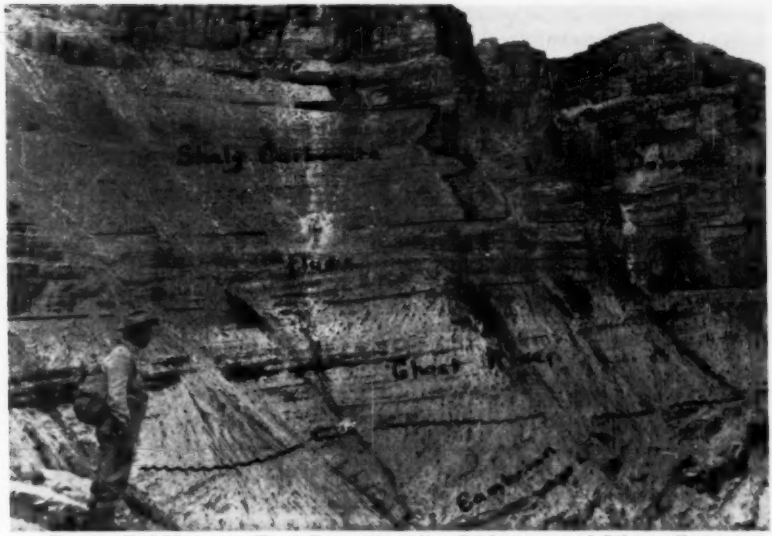

Fig. 1. End Mountain, Front Range, Canadian Rockies, west of Calgary illustrating:
(a) The *continuity of strata* in the Upper Devonian irrespective of the change in lithology from vuggy dolomite on the right to shaly carbonate on the left of the heavy transgressing line.
(b) The depositional absence of the greater part of the Middle and all of the Lower Devonian strata known elsewhere in the subsurface.
(c) The termination of Middle Cambrian strata at the sub-Devonian unconformity.

The name of a stage, therefore, was available for universal application wherever strata of that same stage were known to occur.

D'Orbigny's stratigraphic zone is more familiar from its adoption and refinement by Oppel as a subdivision of a stage. A zone was named by both d'Orbigny and Oppel after a guide fossil, which has commonly led to the misunderstanding that a zone is something radically different from a stage. The difference, however, is only in degree, not in kind, for as Oppel himself says, zones could equally well have been named after places (Arkell, 1933, p. 17). As with a stage, the recognition of a stratigraphic zone is dependent on the presence of some evolutionary or otherwise diagnostic criterion of more than local occurrence be it faunal, depositional or both.

A stage is commonly and incorrectly held to be a paleontological unit because it is recognized universally by fossil criteria. This view is not complete, however, as it omits to indicate that the paleontological unit varies in stratigraphic (vertical) range in different places, and that the total range must be established by adding-up all of the different local ranges. Furthermore, owing to the natural discontinuous nature of facies criteria, certain non-fossiliferous strata which are contemporaneous with the faunal unit must also be included in order to recognize the presence of strata representing the stage in question. An illustration is Oppel's own recognition of a certain ammonite zone where it

is represented only by the occurrence of spongy limestones (Schindewolf, 1957, p. 397). Still other conformable strata above and below the faunal unit should be included (if reasonable) within the natural, distinct, stratigraphic unit (fig. 2).

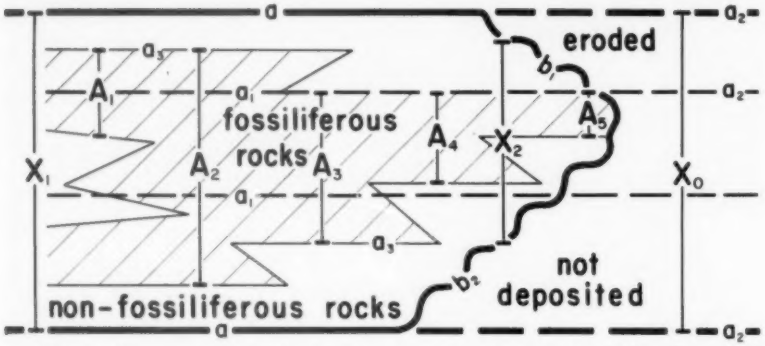

Fig. 2. Diagrammatic sketch of a "stage"—and various criteria which make up such a stratigraphic unit.

X_o = hiatus representing complete stratigraphic unit X_1.
X_2 = incomplete stratigraphic unit.
A_1, A_3, A_5 = different local ranges of guide fossil.
a, a_1 = inter-stratal planes of contemporaneity.
b_1 b_2 = inter-stratal time-significant unconformities.
a_3 = facies boundaries transgressing strata.
a_2 = hypothetical time-planes (of hiatus X_o) corresponding to inter-stratal planes a, a_1.

Many geologists use the term stratigraphy in an extended sense, which from the etymological point of view is more correctly stratology (*strata* and *logos*—study of). An example is found in Gignoux's statement, "Stratigraphy, that is to say, the description of these successive panoramas, will then appear to them (professional geologists) as a harmonious succession of coherent geographies; it will then truly deserve the name of Science." (Gignoux, 1955, p. x).

The acceptance of this combination of objective stratigraphy and logic may perhaps be attributed to the concept of facies developed by Gressly (1814-1865). From his mapping in the Solothurn Jura, he concluded that different conditions of sedimentation must have been responsible for the observed differences in rocks and fossils which obviously were deposited simultaneously. Similarly and independently, but on theoretical bases, Prevost in 1838 explained that we should find in the stratal column contemporaneous deposits of terrestrial, littoral, marine, etc., origin replacing one another from place to place (see Zittel, 1895, p. 502-503; Teichert, 1958b, p. 2719-2721).

The facies concept has been responsible for a rather agnostic attitude among geologists as to the value of Smith's, d'Orbigny's and Oppel's fossil criteria in stratigraphic subdivision and correlation. Instead, it should serve to stress factors other than the fossil record which were taken into account by traditional stratigraphers in correlation. As Lapworth stated in counter to Barrande (Watts, 1939), "There are two valuable warnings with which the

future student of historical geology could ill afford to dispense. One is the fruit of paleontology without stratigraphy—the other of stratigraphy without paleontology". The facies concept emphasizes the mutual relationship of paleontology and stratigraphy, and also serves to show that correlation is possible despite facies change. Johannes Walther enuniated such a feature of sedimentation as a principle in the correlation of facies (fig. 5):

> In a single sedimentary series, the variations of facies that are observed about a given point, reproduce in general the variations of facies (within) strata which follow vertically from the point. (adapted from Gignoux, 1955, p. 14)

The validity of Walther's law in correlation depends on the concept of facies variation (change). Both Gressly's and Prevost's conclusions were from considerations of place and time. It has been the practice to speak of contemporaneous facies as "facies changes" or as "facies of one another", although stratigraphic facies, or stratigraphic equivalents, are terms which seem to be self-explanatory.

From the time of the Second International Geological Congress in 1881, traditional stratigraphic concepts have been universally accepted. This may be seen in the tables of stratigraphic classification of the Second and Eighth Congresses of 1881 and 1900, the tables of Schenck and Muller (1941), and Dunbar and Rodgers (1957, tables 15, 16, and 18, p. 291-292). These tables are based upon the principles that the relationship between rock and time is

GEOLOGIC TIME UNITS	STRATIGRAPHY FORMAL UNITS	STRATIGRAPHY informal units
ERA	GROUP	stratigraphic formations OR correlation units
PERIOD	SYSTEM	
EPOCH	SERIES	
AGE	STAGE	
———	ZONE	

Fig. 3. Chart showing traditional terms for Geological Time Units and Stratigraphic Units. Formal and informal stratigraphic units are of same kind, but the formal are internationally recognized while lack of sufficient evidence in places allows informal reference to such units as shown. Terms for contemporaneous facies or arbitrary facies are omitted.

stratigraphy, and the relationship between contemporaneous rocks is correlation (fig. 3). Thus, it has been traditionally accepted that the terms *stratigraphy* and *correlation* have time-significance.

PRACTICALITY OF TRADITIONAL STRATIGRAPHY

On the basis of this understanding the following traditional terms and their usages are favored by the authors because of their clarity and practical

applicability. These terms serve simultaneously the three purposes of stratigraphic classification, correlation and nomenclature. For example, the stratigraphic unit is classified as the basis for (1) systematic subdivision; (2) correlation through contemporaneity; (3) this unit may be informally termed "stratigraphic formation"—for detailed mapping.

(a) *Stratigraphy.*—This term has a very specific meaning in geology and therefore it is unsound to expand this meaning to apply to two or more categories of criteria. The term should not be applied to arbitrary facies criteria, nor to paleontological units which are only part of stratigraphic units. Nor should stratigraphy be applied to abstract time—except in a purely philosophic manner.

(b) *Correlation* in stratigraphy should always imply time-significance. It is the recognition of contemporaneity between different facies, either locally, regionally, or inter-regionally. This concept precludes correlation based upon lithologic similarity which more often than not is transgressive with respect to the stratal succession. If time-significance is not intended, then "lithologic comparison", or "tracing" of a particular lithology in outcrop, or in the subsurface are more correct expressions. Besides recognizing the stratigraphic unit as the basis for subdivision of a succession, it is possible to establish on purely physical criteria these basic intervals for regional correlation analogous with a stratigraphic zone or stage.

(c) *Stratigraphic unit.*—This term must be used when and where time-significance is intended. Confusion will be avoided if this term is not used for arbitrary facies criteria, which by themselves are not necessarily time-significant. The stratigraphic unit is not usually characterized by a typical lithology as generally is the American "formation", but rather is typified by its characteristic lithologic variations. On a regional scale, the unit can be recognized as the smallest, distinct interval within which contemporaneous facies (lithologic, faunal, environmental) can be correlated (figs. 2, 4, 5). Stratigraphic units are bounded by real, observable, time-significant horizons which are planes of contemporaneity or unconformity. Because the unit is not complete everywhere, it represents either all or only part of a certain interval of geologic time.

Where complete (fig. 4), the stratigraphic unit is bounded by horizons which approximate to planes of contemporaneity.[3] Where the unit is absent, the consequent hiatus corresponds to the complete stratigraphic unit (not to a hypothetical time interval), and the upper and lower boundaries of the unit then coincide with the abstract time-planes representing the boundaries of the hiatus. Thus stratigraphic units are physical entities of rock which are more representative of certain intervals of time than are most other criteria. For example, paleontological units are useful in age determinations but these may or may not represent certain intervals of time as completely as do stratigraphic

[3] Caster's (1934, fig. 2) diagram of terms for stratigraphic and facies criteria is very realistic and simple. It is, however, not adequate and may have been misleading in that the stratigraphic unit as illustrated, pertains only to where the unit is known to be most complete. Thus, the definition of time-stratigraphic unit is also inadequate, as it, as well as Caster's illustration, should take into account the same stratigraphic unit where it is not complete, (fig. 4) and is bounded by unconformities.

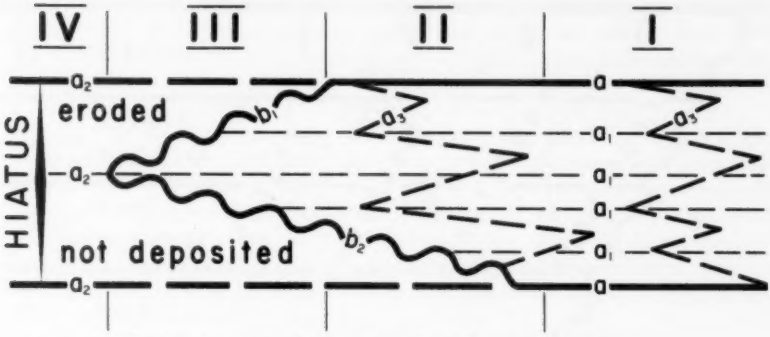

Fig. 4. Stratigraphic or correlation unit under different circumstances:
I = complete stratigraphic unit where bounded by planes of contemporaneity;
II = incomplete stratigraphic unit with two different boundaries;
III = incomplete stratigraphic unit bounded by two unconformities. See Fig. 2 for meanings of letters.

units. Paleontological units therefore, must not be termed stratigraphic units and should not be confused with these. A stage is a stratigraphic unit and not a paleontological unit.

Although all stratigraphic units are analogous in kind, they are of different rank. It is useful to discriminate between the internationally recognized or formally established and named units, system, series, stage, and zone, and the informal stratigraphic units encountered everyday and whose relationships to the formal units are unknown (figs. 4 and 5). To illustrate our concept in practice, the Nisku formation of Alberta is Upper Devonian in age, but because it is not known which specific zone(s) or which part of the Frasnian stage it may represent, it is not practical to call it a stage or a zone, but the Nisku stratigraphic unit, or more briefly, the Nisku formation.

(d) *Stratigraphic horizons* (figs. 2, 4 and 5) are time-significant boundaries of distinct intervals in the stratigraphic succession. These boundaries may be sedimentary—approximating planes of contemporaneous deposition (Caster, 1934, fig. 2) or they may mark erosional or non-depositional surfaces of unconformity at varied angles to the bedding. The term stratigraphic boundary or time-marker is thus applied strictly to markers between strata and not to transgressive facies (formational) boundaries which occur within or across strata. *Planes of Contemporaneity* are lithologic marker-horizons which may have local or regional distribution. Generally they are sub-parallel to one another and parallel to local and regional bedding planes. They are caused by regional tectonic influences which produced relatively short-lived contemporaneous deposition, erosion or non-sequence, e.g., the marker horizons of Porter and Fuller (1959). Regionally, therefore, these are real, practical time-markers (as opposed to time-planes). Along the margins of sedimentary basins, where erosion and non-deposition were more effective, the planes of contemporaneity may develop into unconformities related to hiatuses corresponding to several tens or several hundreds of feet of stratigraphic section known where the suc-

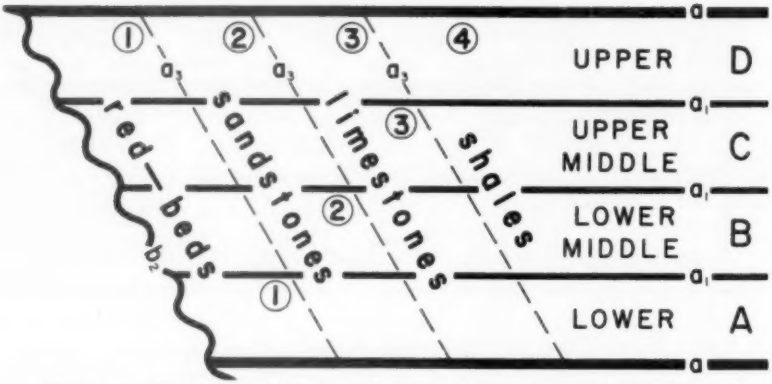

Fig. 5. An illustration of Stratigraphic Subdivision and Nomenclature. Also, shows Walther's Law of Correlation of Facies in its simplest form.

A, B, C, D: Stratigraphic units demonstrating superposition; each unit may be termed a stratigraphic formation, or A+B+C+D may be termed one stratigraphic formation with four vertical subdivisions, each with time-significant boundaries.

1, 2, 3, 4: Different stratigraphic (contemporaneous) facies which may be termed different lithologic members of each stratigraphic formation.

a, a_3, b_2: See figure 2.

cession is more complete. Thus, unconformities are also time-significant stratigraphic boundaries, but they are not planes of contemporaneity. A stratigraphic unit may be bounded by a combination of such various stratigraphic horizons (fig. 4).

The regional extent of these time-significant stratigraphic boundaries makes them often more valuable in regional correlation than are fossils or lithologies which invariably are less widespread. These latter criteria must be related to the stratigraphic boundaries as long as these persist, but when these are indistinct, fossils are useful to identify the specific stratigraphic unit.

(e) *Facies.*—Qualifying adjectives or manner of expression are applied to this general term for specific usage. "Stratigraphic facies units" means different contemporaneous facies, or different facies which are stratigraphic equivalents. The alternate traditional expressions "facies change" and "facies of one another" also have this specific connotation by virtue of the original derivation of the facies concept from the mapping of stratigraphic units.

(f) *Formation.*—This term has a broad meaning. When the term is used without an accompanying qualifying adjective it is invariably arbitrarily and naively applied, thereby creating ambiguity in understanding. Woodring's (1953, p. 1082) remark that "there should be no argument as to what a formation fundamentally is" is insignificant if he does not indicate whether his is a stratigraphic formation or a facies formation. Both of these are based strictly on real, objective, lithologic criteria. The former, however, is based on time-significant lithologic features which do not transgress stratal boundaries. The facies formation may be a facies of one, or of several stratigraphic units, and

need not be considered incompatible with time-significant criteria. In fact, the unsystematic proliferation of facies formational names can be controlled quite reasonably by naming only the specific stratigraphic unit as a formation which incorporates the facies units as members, tongues, reefs, etc., designated according to lithologic type, e.g., Nisku dolomite versus Nisku shale.

(g) *Zone.*—In stratigraphy this term has a very specific meaning which need not be confused with hospital zones or mineralized zones, and other such zones. A zone is not a paleontological unit but its recognition universally is based on its characteristic fossils—as is recognition of a stage. A zone is no different in category from stage, series and system; the only difference is in hierarachal rank. Why should a zone differ from the other three ranks of stratigraphic units when these latter consist of zones? A zone, stage, series, system and group are not paleontologic units and should not be confused with geological time units. The latter are bounded arbitrarily by abstract time-planes which should not be confused with real stratigraphic boundaries.

IMPRACTICALITY OF MODERN STRATIGRAPHY

The following modern terms are not recommended because their meanings in practice do not coincide with their definitions nor with their euphoneous implications. These terms in theory place superfluous stress upon the means for determining conclusions, whereas, in practice, these means are actually the conclusions themselves. The plausibility, simplicity and utility claimed by proponents of modern terms fail to hold when driven to logical conclusions because they are based on fundamental distortions of the truth.

(a) *Litho-stratigraphy, rock-stratigraphy, or rock-units.*—These terms are supposed to imply that the stratigraphic study and conclusions arrived at are based solely on lithologic criteria believed to be dominantly objective; that is, without time-attributes or fossil evidence which are considered to be interpretive or dominantly subjective. This intended meaning is fallaceous and correctly reflects a pre-mature or pre-stratigraphic understanding of the use and significance of these criteria, but there is no need to becloud the situation further by using the term *stratigraphic* when it is uncertain which criteria are in fact stratigraphic and which are only facies. It follows, therefore, that neither the term *rock-stratigraphic* nor its definition are realistic or adequate, and consequently its utility is seriously questioned.

The Illinois State Survey has used the term rock-stratigraphic in the time-significant sense. This use, and the term, are not favored because of its common misuse in the facies sense which has assisted in the loss of the perfectly sound and adequate term stratigraphic unit or stratigraphic formation for time-significant criteria.

(b) *Bio-stratigraphy.*—This term as generally employed is supposed to suggest that the stratigraphic studies and conclusions arrived at are based solely on fossil criteria which previously were considered to be dominantly subjective. In general practice, however, bio-stratigraphy covers both guide and facies faunas without recognizing that these are distinctly different but mutually useful types of criteria. Another misconception attached to bio-stratigraphic units is the belief that these are synonymous with so-called time-stratigraphic

units and therefore, the boundaries of both kinds of units are planes of contemporaneity. This misconception was illustrated at Dallas in March, 1959, when Wilson of Texas University concluded that the top of his time-stratigraphic Arikareean stage did not coincide with the plane which represented the top of his migrating evolutionary *Merychippus* bio-stratigraphic unit. Thus, it is evident that bio- and time-stratigraphic units and their boundaries need not be considered synonymous as suggested by Jeletzky (1956, p. 699), whose remarks probably would apply to the sum-total of faunal and stratigraphic criteria on a global basis which should be one of our ultimate goals for convincing others that some global standards are practical.

If the term bio-stratigraphy is to be used at all, it should be regarded as stratigraphic paleontology which is quite distinct from facies paleontology or bio-facies.

(c) *Time-stratigraphy*.—This term as usually employed is intended to imply that stratigraphic studies and conclusions reached are based exclusively on time criteria which are considered to be dominantly subjective in character. This term is applied by followers of the American Stratigraphic Commission, to simply stratigraphic units of the Second International Geological Congress of 1881. In theory however, Sloss, et al. (1949), Hedberg (1958), Wheeler (1958a), and others conceive of group, system, series, stage and zone units as having boundaries which are planes of contemporaneity only, and have concluded that these boundaries are parallel to, and coincident with the abstract time-planes of the theoretical Geological Time units. Because these workers have not been able to demonstrate this misconception in practice, having failed to appreciate that there really are objective time-attributes in the rocks, they have erroneously considered their time-stratigraphic units as subjective. This basic premise of these authors is, therefore, quite unrealistic and inadequate, and the prefix "time" is superfluous because it places undue emphasis upon time-attributes in the rocks where their presence in fact has not been appreciated by these workers. Because the basic premises in time-stratigraphy are not correct, the related concepts are consequently ambiguous and of little practical value in stratigraphy proper.

TRADITIONAL AND MODERN CONCEPTS COMPARED

The basic difference between traditional and modern stratigraphic concepts is that the former recognize in stratigraphy a "harmonious succession of coherent geographies" (Gignoux, 1955) whereas the latter consider stratigraphy as an "arbitrary partitioning into arbitrary units" (Hedberg, 1958b, p. 193). Many reasons produce these different attitudes toward stratigraphy. The main reason, however, is that traditional procedure is based on thorough studies which have led to proved and systematic conclusions whereas modern procedures are often derived from reconnaissance work. It follows from this arbitrariness that modern schemes for classifying and naming stratigraphic criteria are unsystematic and ambiguous. To make matters worse the ambiguity has been increased by the indiscriminate use of traditional terms for example, *stratigraphy* for time-significant criteria and for facies criteria with no time-significance. It is because of the arbitrary attitude of modern stratigraphers

that the stratigraphic language has become so ambiguous. Indeed, the traditional stratigrapher has become almost as ambiguous and indiscriminate as his modern counterpart by acceding to the use of modern terms and definitions because of a naive reluctance to openly disagree with modern concepts. We therefore, do not agree with the claim that modern concepts provide a systematic code for all stratigraphic purposes. We agree with Schindewolf (1957) that modern concepts and procedures are very essential but they are in reality pre-stratigraphic.

An example of traditional concepts of stratigraphic criteria and their relationships may be seen in the illustrations by Muller and Schenck (1943, figs. 6 and 7, p. 272-274). By contrast, modern concepts of the same criteria are illustrated by Hedberg (1958, fig. 2, p. 205), and Wheeler and Mallory (1956, figs. 4 and 6, p. 2715), and Wheeler (1958, figs. 2 and 4, p. 1052-1053). It is to be noted in particular that Muller and Schenck stress firstly the time-factor (i.e. stratigraphic units and their boundaries); secondly, the pertinent fossil criteria, and lastly, if at all, the lithofacies criteria (fig. 4). The other authors, however, arbitrarily stress the relative importance of these different criteria in reverse order, thus illustrating further the difference between traditional and modern concepts. Whereas the traditional stratigrapher sets his sights high by regarding attributes of time as the guiding factor in stratigraphy, modern workers drift aimlessly about in their arbitrary philosophy that lithologic, biologic and time criteria should be treated without simultaneously relating one with the other.

The confusion which has resulted from the ambiguity in modern stratigraphic language may be illustrated first by "rock-stratigraphic". It is generally agreed that in subdividing a local geological section it is necessary to use objective rock-units and their boundaries. In practice, however, there are found three distinctly different categories of objective rock criteria: (1) planes of contemporaneity, (2) planes of unconformity, and (3) litho-facies boundaries, yet, all of these are termed "rock-stratigraphic" (figs. 2, 4 and 5). It is, therefore erroneous and certainly misleading to imply that these time-significant or stratigraphic criteria are "rock-stratigraphic", which is a rather unfortunate way for the Illinois Survey to 'agree' with the Stratigraphic Commission.

Modern usage of the term rock-stratigraphic is misleading because its etymology and euphony suggest that, in the traditional sense, it is simply synonymous with stratigraphic, or according to modern vernacular, with time-stratigraphic. Modern stratigraphers (Schenck and Muller, 1941), began misusing the term stratigraphic when they misapplied it to litho-facies criteria and incorporated it into their 'code' for their own usage. The 'official' approval given this ambiguous usage by the Stratigraphic Commission has left the traditional stratigrapher with neither the term itself, nor with the original definition for, simply—stratigraphic criteria. The unavailability of the term rock-stratigraphic for stratigraphic criteria has led Forgotson (1957, p. 2108) to the shrewd and realistic observation that

> The work of stratigraphers making regional correlations and mapping rock units on a regional scale is hindered by the lack of a suitable terminology. The relation-

> ships of the vertically segregated rock units in one area to equivalent units in areas where the lithologic character is different, can not be adequately expressed by the present system of stratigraphic nomenclature.

However, Forgotson's proposal of the term *format* and the Illinois State Survey's recapture of rock-stratigraphic for its more realistic use, do not fully solve the problem. These workers fail to realize that the lack of a suitable term must be attributed to modern stratigraphers who have claimed the term for their own system of stratigraphic nomenclature wherein the originally suitable term has been rendered unsuitable. It is useless to propose new terms when former terms for the same criteria are presently being misused for other criteria.

Bio-stratigraphic correlation has been considered to be subjective[4] because it is useful in establishing time-stratigraphic boundaries which are also condemned as subjective. Jeletzky (1956, p. 688), in rightly criticizing this view, states:

> In this connection, it is proposed that the unfortunate and misleading term, biostratigraphy be dropped altogether. Under the definition of stratigraphy proposed in this paper, this term becomes highly misleading and practically synonymous with 'bio-facies'.

Thus Hedberg's claim that one term should not be used for several purposes is a paradox upon modern usage of the terms litho-stratigraphy and bio-stratigraphy.

The insistence that stratigraphers should abide by the arbitrary rules and ambiguous terms and definitions of modern concepts is unfortunate, and the fallacy of such unrealistic procedure is demonstrated by innumerable examples similar to the following from Harker, Hutchinson and McLaren (1954):

> Sloss and Laird (1947, p. 1427) report that conodonts from their Unit C of the Devonian in northwestern Montana were considered of Late Devonian (Senecan) age by C. L. Cooper (1946, p. 612). On lithologic grounds they believed that unit to be in part a correlative of the Ghost River of Alberta. Webb (1950-51, p. 2299) regarded this formation as the lithologic equivalent of the sub-surface Elk Point of which the upper part at least is Middle Devonian. Andrichuk (1951) brings forth cogent evidence based on lithology, in support of this correlation.

Here, it is quite apparent that "lithologic similarity" is the dominantly objective factor in the otherwise impossible correlation of Upper Devonian with Middle Devonian strata.

In addition to the unrealistic concept of 'correlation' based on lithologic similarity, there is a strong tendency for stratigraphers and paleontologists to sanction another form of impossible correlation. This is the practice of including an unconformity within a formation as it is generally understood (Woodring, 1953, p. 1082). This procedure neglects the fact that an unconformity may mark a break in the continuity of the stratigraphic record which is much more important than the fact that there may be lithologic similarity above and below the unconformity. The damaging aspect of this procedure is that on the basis of one guide fossil below an unconformity an entire formation all too often has been assigned to one stage (fig. 6). Consequently strata separated by the unconformity are considered to be equivalent in age whereas

[4] It is not known whether the strong arguments of Jeletzky and others have brought about a reversal of opinion, or whether Hedberg (1958, p. 195) and others are actually convinced, but bio-stratigraphy is now considered dominantly objective.

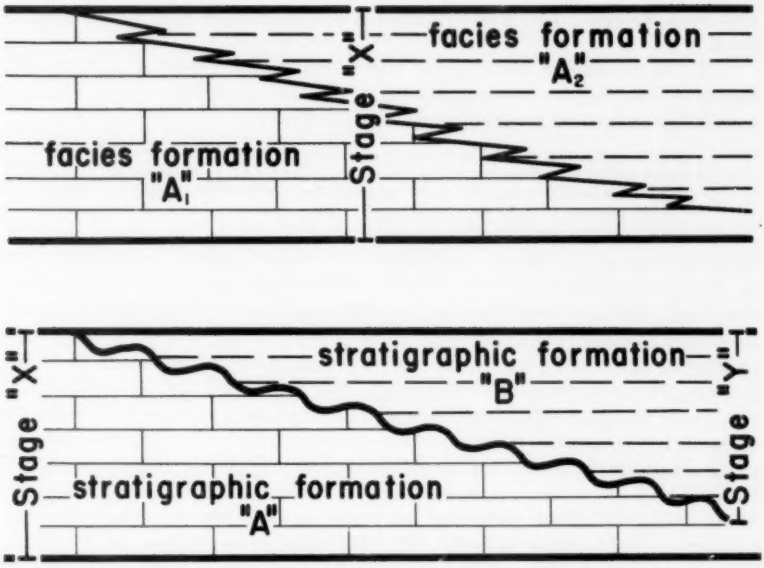

Fig. 6. Diagrams to show that in bio- and litho-stratigraphy which do not discriminate between arbitrary facies and time-significant criteria, unconformities and other stratigraphic horizons (lower diagram) are often wrongly depicted as facies boundaries (upper diagram) with resultant misconceptions in correlation (chrono-stratigraphy) in upper diagram. Upper diagram represents an example of pre-stratigraphic conclusions frequently derived from modern concepts, and true stratigraphic conclusions in lower diagram. Hence this figure illustrates the uselessness of controversy between modern and traditional concepts in stratigraphy.

in fact their deposition may have been separated by many epochs or even periods in time. It is serious violation of stratigraphic principles, to consider that a regional unconformity occurs within a stage. It is argued, therefore, that any effort to mask an unconformity is considered unrealistic in matters of correlation.

The importance attached by modern workers to lithologic similarity has led to considerable ambiguity in modern schemes for nomenclature of stratigraphic units such as group, formation, member, etc. The application of these names to a particular rank admittedly is a matter of personal, arbitrary choice. Thus, modern schemes for ranking and naming these units are not systematic, nor should they be expected to be so in view of the fact that modern workers do not attempt to refer transgressive litho-facies criteria to specific and consistent inter-stratal features. Another characteristic which erroneously is believed to be attributed to a formation only is its mappability. Actually this part of the definition is also unrealistic because discrete stratigraphic units (format of Forgotson), whether they consist of one or more contemporaneous lithologies, are also lithologic and often better suited for most mapping purposes because of their greater areal distribution. Thus the phrase, "a formation is a mappable unit" can not be logically considered applicable only to a lithofacies

unit, as it is equally and probably more realistically applicable to stratigraphic units.

At the expense of denying the traditional stratigrapher the right to map objective, observable stratigraphic units, Hedberg (1958, p. 203-204), in presenting arguments for modern concepts, states:

> Just because the age is the same is no reason for uniting totally different lithologies under the same formation name . . . . The geologic map picture is not complete without showing both the arrangement of rocks with respect to lithology and the arrangement of rocks with respect to time of origin, but it is impossible to do both with the same units.

These statements can hardly be considered useful arguments based as they are on the false premise that observable facies lithologies are more objective than observable lithologic evidence for superposition and contemporaneity which modernists disdainfully call subjective or interpretive. It is precisely this false premise of subjectivity and consequent misconceptions of stratigraphic classification, and indiscriminate terminology, that has caused the appalling confusion in stratigraphy. There is absolutely nothing wrong with showing both the different lithologies and their containing stratigraphic units simultaneously by using simply one name for the stratigraphic unit, e.g. Nisku, and another name for the lithologic facies, e.g. shale. Geologists who have not been aware of this impossibility have been doing just this thing for over 150 years. For example, Cobban's (1945) and Imlay's (1952) Jurassic formations of Montana, Wyoming, North and South Dakota, and the Devonian formations of Alberta as proposed by Imperial Oil's geological staff (1950) are such stratigraphic formations.

The time-worn song that "a formation is a rock-stratigraphic unit" is ambiguous. Both terms are unrealistic and indiscriminate because their meanings are too general without specific qualification. The dangerous part of such usage is that it denies the greater usefulness of true stratigraphic units. The authors, therefore, refuse to accept the remarks in Report 5 (p. 1886) that, "The term zone is not a time-stratigraphic term. It is not a subdivision of a stage". The sponsors of such concepts obviously are not thinking stratigraphically, but rather in a manner akin to arbitrary zonation into arbitrary lithofacies units. If these sponsors are not aware of the real distinction between stratigraphic paleontology and facies paleontology, they obviously cannot appreciate that guide fossils provide criteria for the basis of the stratigraphic zone. We agree with R. C. Moore's commentary on Report 5 (p. 1888) where he states that, "The most cursory acquaintance with published literature demonstrates that the position of our Commission in this matter [of zones] is utterly unrealistic. Accordingly, ... I must vote as emphatically as possible against it".

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[American Journal of Science, Vol. 257, December 1959, P. 722-733]

SEPARATION OF STRATA: PALEOZOIC LIMESTONES OF THE WILLISTON BASIN

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ABSTRACT. The recognition of thin, persistent marker beds in the Mississippian rocks of the Williston Basin led to a logical separation of these rocks and illustrated the inadequacies of previous subdivisions. Present in number in the dominantly carbonate Lower Paleozoic succession in the basin, similar marker beds have been used to yield an understanding of the stratigraphy of these older rocks.

Here termed *non-sequential beds*, the marker beds are clastic interruptions in an carbonate and sulphate succession which commonly exhibits rhythmic variations. They would appear to be the tangible expressions of diastrophic pulses which terminated periods of widespread standstill in sedimentation. The non-sequences marked by the non-sequential beds in the Paleozoic column of the Williston Basin delineate a number of para-time rock units which the writers choose to refer to as *beds*.

Irrespective of terminology, the method of separation of strata should prove applicable and rewarding in cratonic basins where carbonate-evaporite deposition far exceeds sand-clay deposition.

INTRODUCTION

A statement of views which have evolved over several years among some of the geologists working in the Williston Basin Paleozoics is long overdue. The theoretical considerations outlined in the second part of this paper grew mainly from an endeavor to comprehend the existence of huge sheets of nearly uniform sediment which seemed to have been generated very largely within the area where they were deposited.

The geographical setting and areal extent of the Williston Basin is indicated in figure 1. The highest closing contour on a structure map of the base of the Devonian is used to delineate the basin. Thus drawn, it includes large tracts of Saskatchewan, North Dakota and Montana, and smaller areas of South Dakota, Manitoba and Alberta: a total area in the order of 125,000 square miles. Sediment totalling some 15,000 feet in thickness accumulated at the center.

Through a considerable portion of its history, from Middle Ordovician to the close of Paleozoic time, the Williston Basin existed as a relatively simple interior basin, an area of negative tendency on an otherwise stable platform. Later in time it lost its initial character, becoming a more complex and diversified depositional area influenced by orogenic activity in the Cordillera. During the time in which the Paleozoic sediments (with which we are here concerned) accumulated, however, the Williston Basin was intracratonic (Krumbein and Sloss, 1951, p. 337) or autogeosynclinal (Kay, 1951, p. 17) that is to say, a basin in which the rate of sediment accumulation characteristically was slow and delicately balanced against the rate of subsidence.

The rock layers of a sedimentary column, such as the Williston Basin Paleozoics, may be divided one from another by a variety of discontinuities ranging from the smallest perceptible break, usually the bedding-plane, to the largest disconformity, or, with the development of angular discordance, to the unconformity. At one end of the range of discontinuities, the bedding-plane marks the passage of an instant of time—but of an "instant" measured perhaps

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Fig. 1. Geographical setting of the Williston Basin.

in thousands of years (cf. Read, 1958, p. 89)—and at the other the unconformity, which often marks the passage of several millions of years.

As is well known, the physical effects of events during a sedimentary break are scarcely a reliable guide to the duration of the break, although in practice a degree of correlation between the rank of break and the time-lapse it represents seems to exist. Without recourse to paleontological or chemical evidence of age any estimate of the time-lapse represented by a break may be far from accurate. Nonetheless, in the consideration of any of these discontinuities, the passage of time without accumulation of sediment is the fundamental concept.

CHANGING CONCEPTIONS OF PALEOZOIC STRATIGRAPHY OF THE WILLISTON BASIN

In the study of the Paleozoic sediments of the Williston Basin the importance of marker beds (certain of which the writers call *non-sequential beds*)

has become increasingly evident. Their role in bringing about a proper understanding of the stratigraphy is perhaps most readily illustrated by reference to the Mississippian rocks (figs. 2, 3 and 4).

The Mississippian sediments in the Saskatchewan portion of the basin consist largely of Madison Group carbonates. These are underlain conformably by Bakken Formation clastics (Kinderhookian) and unconformably overlain by post-Paleozoic rocks. Until recently, a threefold formational division of the Madison succession was generally recognized, but the exact positions of the formational boundaries outside the type areas occasioned considerable local controversy.

In figure 2 the datum selected is the base of the lowest anhydrite bed in each borehole. This conforms to the early definition which placed the base of the Charles Formation at that point, and implied for most of us a lateral continuity of any appreciable anhydrite in the Mississippian succession. The dashed line divides the lower two-thirds of the Madison Group column into Mission Canyon Formation above and Lodgepole Formation below. It can be seen in this figure (and in figs. 3 and 4) that the line does not extend across the entire section; on the eastern flank of the basin formational subdivision is impossible.

Figure 3 illustrates a later concept. As exploratory drilling increased, particularly in southeastern Saskatchewan and in north-central North Dakota, the preponderance of anhydrite in the east-central portion of the basin led some geologists to express a belief in the existence of a distinct evaporite basin, and the evaporites were thought to lie unconformably on the carbonates. In this illustration the datum is drawn at the base of the carbonates, and the dashed line again marks the junction of the Lodgepole and Mission Canyon Formations.

With further Mississippian drilling and with the advent of more sensitive electrical and radioactivity logging techniques, it became increasingly obvious that to continue with either of the foregoing interpretations contradicted the detailed correlation between boreholes thus made possible. The new reconstruction rested upon the recognition of thin, persistent, and previously unidentified marker beds; many of them the non-sequential beds of the present paper.

As shown in figure 4, utilization of these beds (one of them constitutes the datum plane of the section) results in a stratigraphic subdivision of the Madison Group rocks, which simultaneously illustrates the descent of the evaporitic facies in a shoreward direction, and the uselessness of the Charles Formation base as a stratigraphic datum. In addition to the datum, six other non-sequential beds have been correlated across the section. For clarity, only six are shown, though many more might have been included. The correlations illustrated are now generally accepted, and it will be realized that only this cross-section (rather than figs. 2 or 3), can show the beds in realistic attitudes and interrelationships.

During 1957 and 1958 the number of boreholes reaching Lower Paleozoic rocks in previously unexplored parts of the Williston Basin increased appreci-

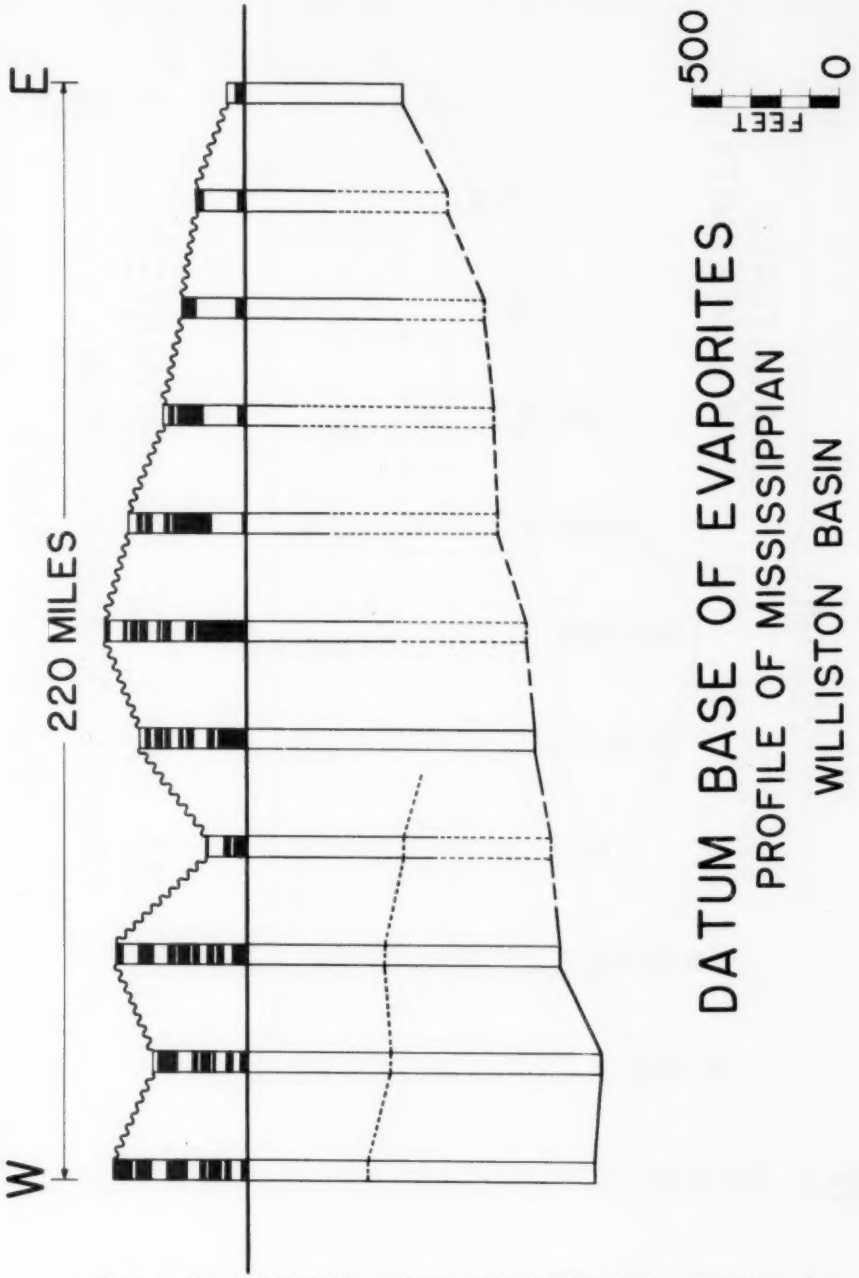

Fig. 2. Profile of Mississippian sediments of the Williston Basin: datum base of evaporites.

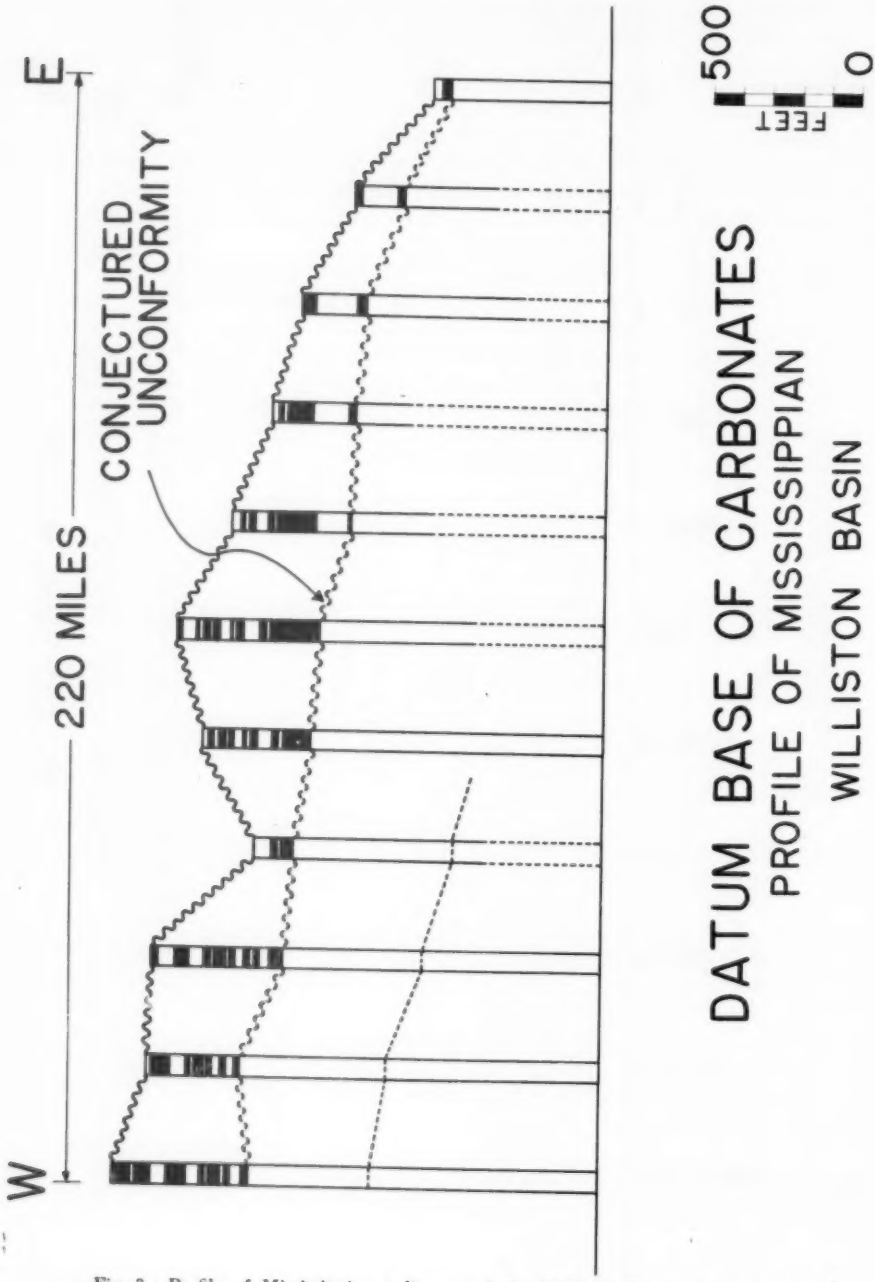

Fig. 3. Profile of Mississippian sediments of the Williston Basin: datum base of carbonates.

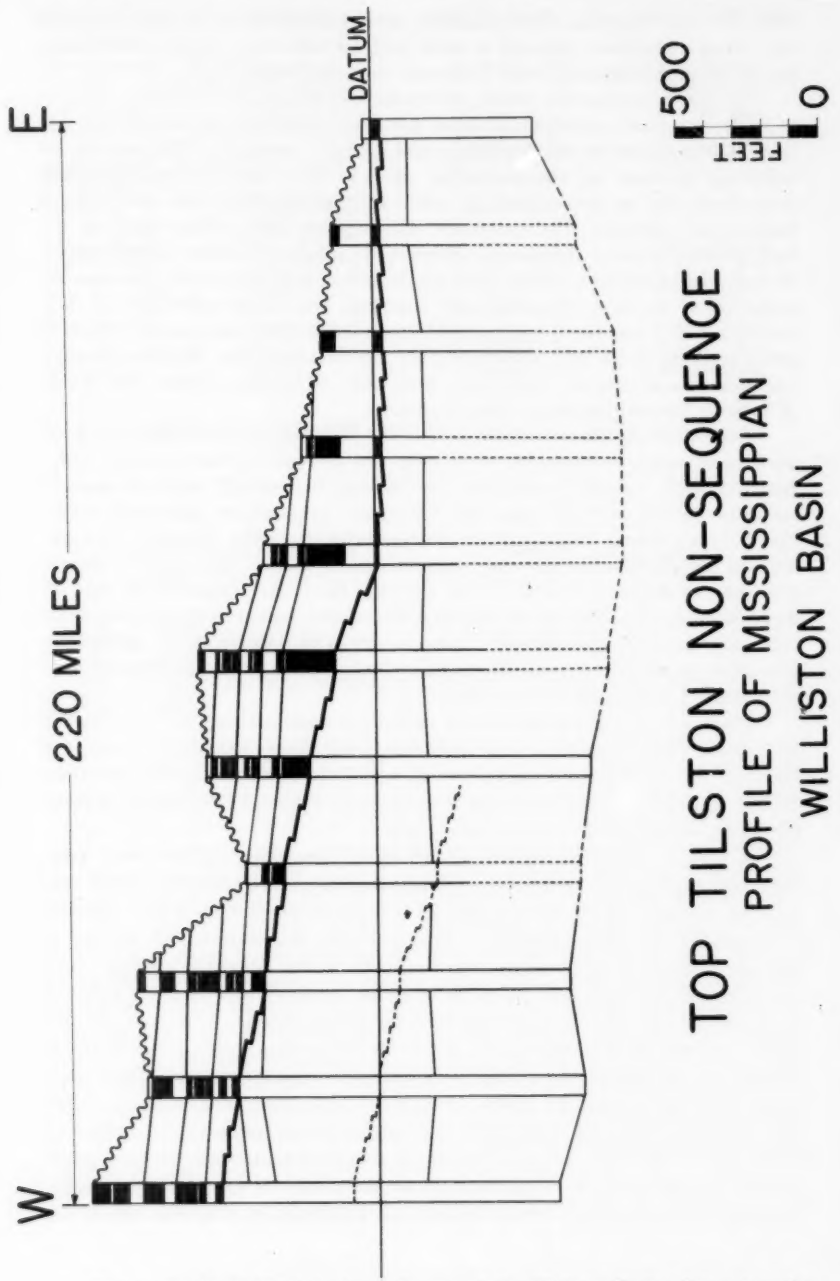

Fig. 4. Profile of Mississippian sediments of the Williston Basin: datum top of a non-sequential bed. (Top of Tilston Beds.)

ably. The new material made available, when considered in the light of Madison Group experience, allowed a rapid and, we believe, a correct understanding of the generalities of Lower Paleozoic carbonate stratigraphy.

NON-SEQUENTIAL BEDS, RHYTHMS AND TIME-PARALLELISM

The practical usefulness of what are here called non-sequential beds has been demonstrated in the foregoing stratigraphic summary. The reasons for referring to them as non-sequential rest on other observations. Generally they stand out as thin seams of exotic lithology in otherwise well-ordered families of sediments. The carbonate and sulphate beds, which comprise the bulk of the Paleozoic succession, undoubtedly originated either directly or indirectly (biochemically) from material dissolved in the sea water, whereas the materials of the non-sequential beds comprise disordered collections of clay, quartz silt and fine sand, coarse highly-rounded frosted sand grains, dolomite pebbles, shale chips and sometimes (in the Mississippian Madison Group) carbonaceous fragments. The clastic beds seem to be out of place, like layers of rubbish laid on courses of clean masonry.

Several sandy non-sequential beds of the Mississippian and Silurian which have been cored or observed at outcrop are strongly current-bedded. Their junctions with underlying beds are very abrupt, occasionally with evidence of current-scouring and abrasion, but the upper contacts are generally gradational. Fine-grained beds, typified by the marly or earthy dolomite interruptions of the Madison Group carbonate succession in the Mississippian, do not yield any evidence of strong current activity; the lower surface often rests on more altered rock, the newer material sometimes penetrating it along what looks like a weathered surface. Other evidence of weathering is yielded by iron-oxide replacement of shells at the contact, and extensive metasomatic replacement of carbonate by anhydrite.

From a practical standpoint the most important attribute of these beds is their association of low thickness with vast extent. One in the Ordovician (top Gunton) for instance, is not known to exceed 15 feet and is generally less than 10 feet in thickness, yet it extends through more than 100,000 square miles of the basin. At least 30 others are comparable.

A second line of evidence also leads to the conclusion that the thin clastic beds interrupting the carbonate-evaporite succession are properly called non-sequential. Large parts of the Paleozoic column in the Williston Basin exhibit lithologically rhythmic variations, and although the writers are unable at present to abstract an ideal unit rhythm to fit all the Paleozoic systems (such an achievement might turn out to be valueless) several rhythms, each believed to be characteristic of its time, can be constructed (fig. 5). The most complex rhythms, those of the Ordovician, have the (ascending) order: fossiliferous-fragmental (clastizoic) limestone→argillaceous precipitated calcilutites, dolomites and pseudo-oolitic rocks→evaporitic anhydrite→argillaceous (often sandy or pebbly) non-sequential bed→fossiliferous-fragmental (clastizoic) limestone. Evidence is so far inconclusive, but it seems possible that the argillaceous bed beneath the evaporite may mark the end of a rhythm, the underlying limestone probably being represented in places by evaporite (as in the

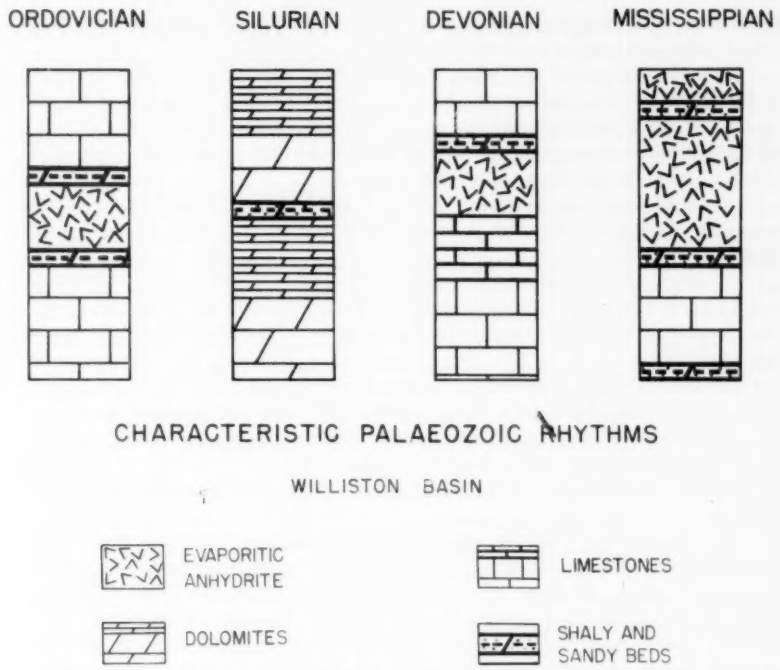

Fig. 5. The general form of Paleozoic sedimentary rhythms in the northern part of the Williston Basin.

Mississippian). Silurian rhythms, by comparison, are poorly differentiated, and lack any widespread evaporitic development. Devonian rhythms include the evaporite, but the underlying argillaceous bed is usually absent. In Mississippian rhythms (on the large scale) this argillaceous bed is missing, and the carbonates display no strong vertical differentiation within rhythms. The argillaceous bed usually present under Mississippian evaporites, however, is thought to represent the non-sequential bed between two rhythms. This contrary view is to some extent supported by the strong lateral rather than vertical differentiation (in contrast to the Devonian and Ordovician) of the carbonate-evaporite facies within Mississippian rhythms. The argillaceous beds under the evaporites can be traced up the depositional dip across a variety of carbonate facies to points where they overlie the evaporites of the preceding rhythms. In this position the argillaceous beds are often rubbly and heavily iron-stained. Incontrovertible rootlet beds have not been found but carbonaceous fragments are common, and recognizable plant remains have been figured in the literature.

The clastic materials overlying the evaporites, particularly the coarse frosted sand grains, shale chips and dolomite pebbles, interrupt well-ordered sequences of rock derived from the sea water itself; without perceptible change the clastic seams cross the facies boundaries of the preceding sequences and

behave as micro-basal-conglomerates to the succeeding sequences. For these reasons also they are termed non-sequential.

Any explanation of the cause of intermittent deposits of foreign material in the limestone succession must be very speculative, but there are a few observations which support the idea of intermittent diastrophism, in the form of relatively sudden changes in the rate of basin subsidence (or relative marginal uplift). The principal observations are as follows:

1) most of the non-sequential beds overlie the latest deposits of carbonate-evaporite rhythms (allowing that the evaporite is the final phase of the rhythm);

2) their extent, but not their thickness in the Mississippian, Devonian and Ordovician varies directly with the extent and development of the associated evaporite bed (the Silurian has no known widespread evaporitic anhydrites);

3) the non-sequential beds, in general, are developed better in peripheral (but not extreme edge) areas than in the central area of the basin; and

4) though the non-sequential beds may be classified, according to their composition, anywhere in the range from re-worked residual muds to allogenic pebble beds, they always behave independently of facies-belts in the strata they overlie.

It is fairly clear that the introductions of clastic material occurred after periods of widespread standstill, represented by bedded anhydrite sheets, or even after periods of nearly static weathering, but before the onset of marine limestone deposition in a new rhythm. It is fairly clear also that the main sedimentary break in each rhythm (not necessarily the longest period of near inactivity) occurred at the base of the non-sequential bed rather than above it.

It seems only a small step to infer that the relative depression of the basin, which allowed repeated overstep of marine on to marginal evaporite deposits, also allowed the spreading out of the sheet of clastic material. Though a movement bringing marine waters back to an evaporitic area may also have rejuvenated hinterland drainage, the clastic material may not have been transported directly from the hinterland, for the marine regression implied by the preceding evaporitic stage may have exposed large tracts of strand to subaerial erosion. This idea of repeated exposure and destruction of marginal deposits during a regression punctuated by weak transgressions has been broached elsewhere (Fuller, 1956b). Undoubtedly the clastic materials have the diverse character to be expected of the accumulated sweepings of an exposed strand: in other words, the non-sequential beds composed of them may be no more than fossilized collections of beachcombings.

Before discussing the class of the stratigraphic units which the non-sequential beds delineate it is rather necessary to examine the growing misuse of the word *isochronous* and the distinction between the terms *synchronous* and *time-parallel*.

Isochronous (as Webster has it) means equal in *length of time*, or occurring at equal intervals of time. Synchronous, on the other hand, means happening at the same time, simultaneous.

It is clear from current stratigraphic literature that *points* in time are in

danger of being confused with *lapses* of time, as though sunrise on Thanksgiving Day lasted twelve months. The American Commission on Stratigraphic Nomenclature (Report 2, 1952, p. 1628) offers this statement: " . . . Time-stratigraphic units differ fundamentally from all other kinds of stratigraphic units in that their boundaries are not drawn on physical properties but are based upon geologic time, i.e., they are isochronous surfaces." The statement is quoted and presumably endorsed by the Committee on Pleistocene (American Commission, Report 6, 1959, p. 667) and the same misuse of the word isochronous appears in Hedberg (1958, p. 1891) who writes " . . . The goal of determining perfect isochronous surfaces (those with exactly the same time value everywhere) and perfect boundaries for time-stratigraphic units may never be attained." In the instances cited, the term isochronous surfaces is used where synchronous surfaces are meant.

The distinction between synchronous (rock) surfaces and time-parallel (rock) surfaces may best be clarified by means of diagrams (fig. 6). In Diagram A a tank is supposed to be fed continuously with sediment from a hopper above. The experiment started on Monday but the operator forgot to renew the charge to the hopper until the next Saturday. The original charge was exhausted on Tuesday, and until Saturday nothing disturbed the experiment. Two lines or planes in the sediment are relevant: that separating Monday's from Tuesday's deposit, a period of uninterrupted sedimentation, and that separating Tuesday's from Saturday's. The sediments representing the end of Monday are both synchronous and time-parallel with those representing the beginning of Tuesday (there being no measurable gap between Monday and

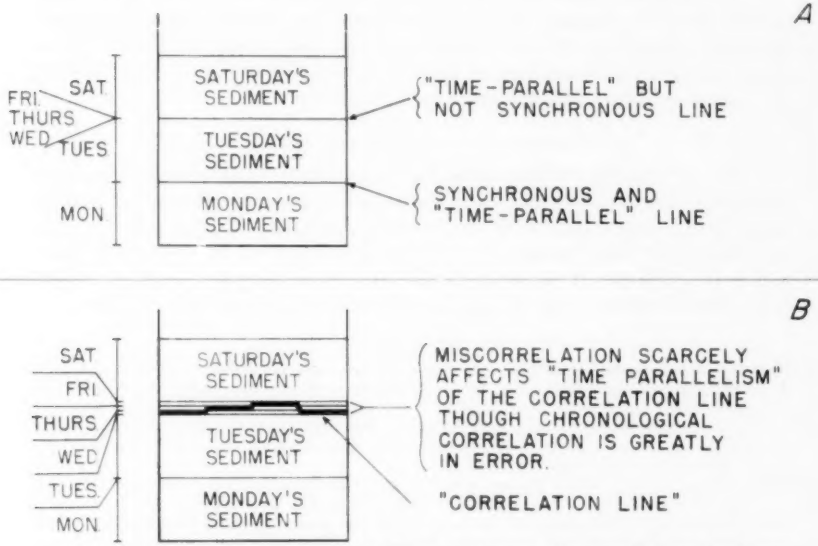

Fig. 6. Diagrammatic illustration of difference between "time-parallel" and synchronous surfaces in a column of sediment.

Tuesday). The sediments representing the end of Tuesday are time-parallel with the beginning of Saturday's, but they are obviously not synchronous with them.

In Diagram B the same experiment is re-enacted, but it is supposed that small amounts of foreign dust entered during the Wednesday to Friday interval. A miscorrelation of the "beds" representing Wednesday's deposition with Friday's would be large in a chronological sense but would scarcely depart appreciably from a time-parallel correlation. For all practical purposes the line would divide two time-sediment units, but to allow of the possible miscorrelation we could call them para-time-sediment units.

If the relationships among the bodies of sediment in this simple experiment resemble the relationships existing between the carbonate-evaporite units and the non-sequential beds in the Paleozoic column, and if the conclusions we have drawn from the experiment are valid, then we might justifiably refer to the stratigraphic units delineated by the non-sequences as para-time-rock units.

It will be noted that the meaning we attach to "para-time-rock" is close to that of Wheeler, et al. (1950) who originated the term. Our method of approach was practical but we reached a similar conclusion.

Stratigraphic units falling in the para-time-rock class are neither ranked nor sub-classed satisfactorily. Moore (1958) recommends "lithizones" or as a second choice "assises", but rejects "beds" (the English equivalent of assises) on the grounds that it is part of the hierarchy of informal rock-stratigraphic classification. He also rightly rejects the Johnny-come-lately "format".

The writers concur with Moore in his repudiation of format, but continue with beds on the grounds of utility, familiarity and (in the English language) historical precedence. As everyone knows, this matter owes its current prominence not to a late perception of geological truth but to the contemporary depreciation of the old para-time-rock term formation.

CONCLUSION: LIMITATIONS AND ADVANTAGES

The conclusions reached in the foregoing review are applicable only to the sediments of cratonic basins where carbonate-evaporite deposition far exceeds sand-clay deposition, that is to say, in areas displaying very low tectonic activity. Subject to these limitations, the spatial connections between small increments of sediment at widely-separated points are revealed, and a realistic interpretation of the vertical sequence of the small increments is possible.

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[American Journal of Science, Vol. 257, December 1959, P. 734-751]

THE ROLE OF FOSSILS IN DEFINING ROCK UNITS WITH EXAMPLES FROM THE DEVONIAN OF WESTERN AND ARCTIC CANADA*

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ABSTRACT. The formation is rightly considered the fundamental unit of rock classification. Definition must be broadly based if flexibility is to be maintained. Fossils as lithologic constituents are equal to any other physical constituent in defining rock units. Their use in this role must not be confused with their interpretive use in biostratigraphic correlation, for which biostratigraphic units are employed. These units are indistinguishable from time-stratigraphic units if the latter are redefined as practical working units; the hierarchy system, series, stage will serve for both.

Three examples are given of the use of fossils in largely reconnaissance mapping of richly fossiliferous Devonian rocks. (1) On Ellesmere and Bathurst Islands the correlation of roughly similar lithologic successions is supported by a sequence of fanuas that allows recognition of the same four formations on the two islands, 200 miles apart. (2) Near Kakisa River, Upper Mackenzie Valley, where owing to poor outcrop, the upper boundary of a formation, that may be seen to correspond with a lithologic break only where the rocks are better exposed, is most conveniently defined by a marked faunal break. Structure was mapped by collecting fossils from small isolated outcrops in the area. (3) In the Alberta Rocky Mountains, difficulties experienced in choosing a consistent boundary between the Upper Devonian Fairholme Group and the overlying Alexo Formation may be solved in fossiliferous sections by using a marked faunal break that commonly corresponds to one of several possible boundaries if lithology is considered exclusively.

In these examples, the meaning of faunal changes or breaks is not relevant to formation definition. Thus no interpretation is involved and the fossils are as much a physical character of the rocks as any other character.

INTRODUCTION

Stratigraphic Nomenclature, in all its aspects, is a subject with a diffuse and rapidly expanding literature, in which there may be detected a considerable and growing body of agreement on major principles. Differences of opinion still exist, however, and many of the papers published on this subject are concerned with summarizing earlier ideas, pointing out divergent views, and arguing for or against one or more aspects of the general problem. This paper makes no attempt to review this literature or to propose new methods of classifying rocks and time. It deals with one small aspect of the whole problem, the use of fossils in formation definition, and makes no claim to originality except in the examples used to illustrate the thesis.

Report 4 of the American Commission on Stratigraphic Nomenclature (1956, p. 2004) states that the boundaries of a rock unit (e.g. a formation) "may be based on minerals or fossils as lithologic constituents, or on grain size, electrical characteristics, radioactivity, transmission of seismic waves, and other properties."

The Stratigraphic Code (Ashley and others, 1933, p. 431) in discussing the formation states: "Nevertheless, a unit distinguishable from the enclosing rocks only by its fossils shall not, in general, constitute a formation, but is properly classified as a paleontologic zone".

The expression "fossils as lithologic constituents" in Report 4 is ambiguous

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and open to interpretation in different ways. But it appears from discussion in the Report and the interpretation put on it by many subsequent authors that it is presumably referring to fossils as rock building organisms, such as coral reefs or crinoid banks. Indeed the Code would appear to rule out fossils as nameable organisms in formation definition (Fisher, 1956), and Report 4 is assumed to endorse this view (e.g. Gray, 1958).

On the other hand, units defined by fossils, biostratigraphic units, are not to be included in time-stratigraphic classification. Because, as is well known, many fossils and faunas are both long ranging in time and tied closely to facies, all biostratigraphic units are considered "relatively objective units, delimited by the actual physical occurrences of fossil forms in rocks. They are quite different in fundamental nature from time-stratigraphic units" (Report 5 of the Commission, 1957, p. 1878).

Hedberg (1958, p. 1883-5, and fig. 1) has lucidly expressed this view in a recent paper in which he reproduces and expands a figure first published in Report 5 (fig. 1). This figure, and Hedberg's discussion, emphasize that there are two fundamentally different groups of stratigraphic categories, the dominantly objective, and the dominantly subjective. Included in Group I, the dominantly objective categories, are lithology (rock-stratigraphic), fossil content (biostratigraphic), mineral content, etc. The subjective categories, which involve interpretation, include time of origin, environment of origin, etc. Grant this two-fold grouping, then all of Hedberg's comments on fossils and biostratigraphic units are logical and follow inevitably.

The thesis of this paper, and it is not new, is that there are two distinct lines of evidence furnished by fossils: their presence in rocks as physical characters, and their interpretative use in correlation. The former is of direct use in formation definition, the latter is not. I am indebted to several of my colleagues for helpful discussion and advice before and during the writing of this paper; especially R. J. W. Douglas, E. W. Mountjoy, G. W. Sinclair, and E. T. Tozer. I am also indebted to Dr. Hollis D. Hedberg for very considerable help in clarifying my views on stratigraphic terminology in general. I take sole responsibility, however, for the views expressed here.

FOSSILS AS LITHOLOGIC CONSTITUENTS

Fossils are collected from rocks. Their presence there is therefore a physical reality. A paleontologist is trained to recognize the different forms of fossils that occur and attempt to identify them. Depending on the preservation of the remains, the experience and training of the paleontologist, and the degree to which similar forms have been figured and described from elsewhere or from the same region, he is able to put names on the specimens he collects. The names may vary through all taxonomic categories from phylum down to species or variety. Although the reliability of names given to fossils must obviously vary, nevertheless a faunal list may probably be classed as being reasonably objective.

In this sense, fossils in rocks are lithologic constituents. They are part of the rock, whether abundant or rare, and, in general, there is normally a reasonable measure of agreement among paleontologists on the names assigned to them.

BIOSTRAGRAPHIC CORRELATION

Given a collection of fossils, and having named them, the paleontologist's work is not finished. He next tries to interpret them, to assess their significance for correlation, to examine their value as indicators of time. This is a very much more difficult procedure, which involves experience and knowledge far broader than that required for naming fossils.

I should like to mention some alternatives open to him: (1) The fauna, coupled with a consideration of the local geology, the lithology and structure of the beds concerned, may allow him to arrive at a conclusion that the fossils were collected from the same formation as another fauna with which he is comparing it. Without his drawing any conclusions as to their age, the fossils thus serve him as additional evidence in extending a rock unit beyond its previously known geographic boundaries. The nature of this judgement can be classified only with a knowledge of the specific case involved, but it is at least commonly dominantly objective. (2) Again, he may decide that the presence of one or two species among the whole fauna indicates that he is dealing with a zone he has recognized previously some distance away. His knowledge of the evolution or the time range of these species may lead him to suggest their age equivalence to the previously recognized zone. This procedure involves the application of hypothesis—an "act of faith", and his judgment is in this case dominantly subjective. (3) Finally, and possibly in addition to (2), he may state that in his opinion the rocks containing the fossils are of Paleozoic, or Devonian, or Late Cretaceous, or early Late Norian Age. This is a purely subjective judgement and depends on application of a series of hypotheses which may well be hard to categorize in detail.

If anyone should doubt that biostratigraphic correlation is a matter of judgment and experience i.e. of the application of multiple hypotheses, let him refer to almost any major paleontological work in which the author discusses the correlation of the rocks from which the fossils he has described were collected. Examples that might be mentioned include Cooper (1957, p. 13-20), and Miller, Youngquist, and Collinson (1954, p. 27-46).

Now I believe that the units used in biostratigraphic correlation are biostratigraphic units and it follows that if biostratigraphic correlation is interpretive, then these units must necessarily be interpretive also. Hence, in the foregoing discussion, example (1) does not involve biostratigraphic units, whereas examples (2) and (3) do. If biostratigraphic units are regarded as objective physical units, in the same category as formations, as suggested by Report 5, then of course they must be kept free from taint of interpretation or subjective judgment. Reference to their actual use in the literature will show that they are, in fact, nothing of the sort. Paleontologic correlation is a difficult and skilled discipline, requiring a high order of experience and judgment. On it depend all intercontinental correlations, most inter-basinal correlations, and, incidentally, the greater part of all the "North American Correlation Charts."

A side effect of classifying biostratigraphic units as dominantly objective categories has caused stratigraphers to shy away from using fossils in defining rock units. Once biostratigraphic units are sharply differentiated from units based on observation and not interpretation, then fossils are freed as valuable

physical criteria, which may be used in defining physical units in the same way as any other physical constituent.

IDEAL VERSUS PRACTICAL TIME-STRATIGRAPHIC UNITS

Biostratigraphic correlation is fundamentally an attempt, however imperfect, at time correlation, and varies enormously in accuracy and refinement. It is essentially interpretative and subjective. The Commission, however, considers the terms system, series, and stage ideal time-stratigraphic units differentiated solely on the basis of time of origin of the contained rocks. Only the zone is left as a unit of biostratigraphic classification.

Rodgers, and many others, contend that the hierachy, system, series, stage, and zone, represents "the best but still very imperfect approximation that practising stratigraphers have been able to make, by using all the criteria for correlation, both physical properties and fossil content, that they can muster" (Rodgers, 1954, p. 657).

In this connection I should like to quote from a recent letter to Nature (Essen, 1957, p. 137):

> The discussion has been somewhat obscured by the fact that some contributors regard units as abstract ideas which are invariable, and others as the practical realization of these ideas which are not invariable. All physical measurements are made in terms of the practical units, and the concept of ideal invariable units does not seem to be necessary although some physicists have found it useful. It is certainly confusing when the same name is used for both the ideal and the practical units, and in my view it is preferable to restrict the term 'unit' to those practical quantities in terms of which the results of measurements are expressed.

This was written by a physicist discussing definition of the second. By changing a few words, but not the sense, this argument sounds remarkably close to Rodgers who maintains "if the term 'time-stratigraphic unit' is to be defined ideally, as in Report 2, then the units that stratigraphers have always dealt with in practice need some other designation" (1954, p. 657). It is comforting to note that other disciplines have their problems concerning definition of fundamental terms.

Essen and Rodgers are concerned with practical units. Rodgers rightly claims that the terms system, series, etc. are time-honored units of practical classification. In my opinion no different terminology is demanded in stratigraphy to differentiate real time from apparent time. One must use the best methods available to distinguish time, and to correlate. As methods of recognizing and correlating systems, series and stages become increasingly refined, so the terms will approach theoretical time-stratigraphic units more closely. Who is to determine or arbitrate on whether they have reached the ideal?

System, series, stage, and zone, therefore, should be defined as practical units in terms of results or usage, and not primarily as ideal units. In Report 2 they are defined as ideal units. In spite of subsequent discussion in the report which suggests that they may be employed as practical units, there is, nevertheless, an implication that a practical unit is something different. This may appear to be hair splitting, but it is important to stress that if time-stratigraphic units are, by definition, practical, then biostratigraphic units become indistinguishable, cannot be given a separate nomenclature, and system, series, etc. return to the fold.

FOSSILS AND FORMATION DEFINITION

I can now return to the main subject of this paper, which concerns one facet of formation definition—the value of using fossils under certain circumstances. Granted that biostratigraphic units are interpretative, the presence of fossils is undeniably a physical characteristic of the rock unit in which they occur and may be of considerable value in defining and recognizing formations. But I would go further and argue that occasions arise when their presence might furnish the main criterion of definition.

The fossils so to be used must be common enough to be easily found, and of a type easily recognized in the field. As emphasized previously, their classificatory rank does not matter. A field geologist may well recognize a faunal change without being able to name the organisms concerned. The physical reality which he recognizes may be put into conventional paleontologic terminology subsequent to the field work by him or someone else. The only criterion to the man in the field is utility—expediency. A unit may be recognized by virtue of its distinctive fauna or the junction between two units defined by a marked and obvious faunal change.

Facies faunas that transgress time when traced laterally for any distance have their exact parallel in other physical attributes in time-transgressive rock units. Appearance or disappearance of a fauna may assist in recognizing a change in facies, as the organism may have been more sensitive to such a change than the present-day observer, who is thus led to recognize a change he might have overlooked.

In small-scale reconnaissance mapping, in an area where limited time and relative inaccessibility demands that the maximum possible use be made of all field observations, fossils may offer valuable criteria on which to base, or with which to supplement formation definition. When outcrops can only be examined briefly and successive observations are widely separated, fossils may prove a unifying factor.

A formation should be defined to do the job required—from small-scale reconnaissance mapping, where similarities are more important than differences, to detailed stratigraphic studies and large-scale mapping, where differences become increasingly important. At one end of the scale a formation may be a single lithologic type of limited areal extent and small thickness, at the other a thick heterogeneous sequence, varying in thickness and relative proportions of constituents, over a wide area.

In the definition must be included the marker-defined, vertically segregated units (formats) of Forgotson (1957), for attribute-defined units may be recognized and delimited by marker horizons (in this connection see Moore, 1958, p. 449).

Ample scope is allowed by Report 4 which considers rock-stratigraphic units as essentially practical units of geologic work that may be defined and their boundaries chosen by physical features, and which may be marked by extreme heterogeneity of constitution. Let us retain this breadth of definition and not destroy its flexibility, and further recognize to the full the value of fossils as physical attributes, equal in value to any other lithologic constituent.

Three examples from rocks of Devonian age in Arctic and Western

Canada are summarized very briefly below. Two describe cases in which faunas have proved of paramount importance in reconnaissance mapping in relatively inaccessible and little known regions. The third presents in outline an example of how faunas may be used to elucidate the spatial relationships of rock units and how they may assist in arriving at a satisfactory and significant rock-stratigraphic classification.

DEVONIAN FORMATIONS BETWEEN BATHURST AND ELLESMERE ISLANDS

In the Queen Elizabeth Islands, which form part of the Arctic Archipelago of Canada, erosion of a fold belt running east-west from Melville to Southern Ellesmere Island (fig. 1) has exposed Paleozoic rocks of Ordovician, Silurian, and Devonian age (Fortier, McNair, and Thorsteinsson, 1954). Eighty thousand square miles were mapped on a reconnaissance scale in 1955 by the Geological Survey of Canada, with a team of eight geologists. During the course of this operation, details of Devonian stratigraphy were obtained at several points along the fold belt and the intervening areas were mapped by means of aerial reconnaissance, short helicopter stops, and the use of aerial photographs.

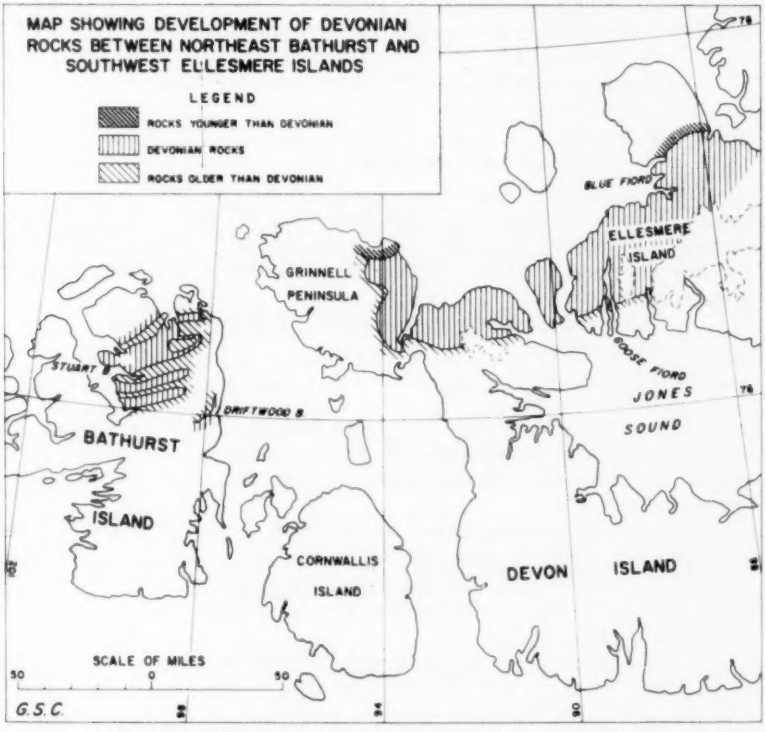

Fig. 1. Map showing development of Devonian rocks between Northeast Bathurst and Southwest Ellesmere Islands.

The Devonian was first studied in the vicinity of Blue Fiord and the adjacent coastline on Ellesmere Island. A stratigraphic sequence was established and divided into four formations, two of which were further subdivided into members (fig. 2). The two middle formations are abundantly fossiliferous with distinct and easily recognizable faunas. To the west and southwest, rocks of somewhat similar type were examined in outcrop in Goose Fiord on Southern Ellesmere Island, Eastern Grinnell Peninsula and Arthur Fiord on Devon Island, and Driftwood Bay and Stuart Bay on Bathurst Island, as well as other areas on Bathurst to the west and southwest. Two of these areas are discussed in greater detail.

In the Blue Fiord area the Devonian sequence is over 17,800 feet in thickness. The base is not exposed, and the lowest beds, with a minimum thickness of 1,000 feet, are assigned to the Eids Formation[1] which consists largely of soft weathering shales and mudstones for the most part devoid of fossils. The overlying Blue Fiord Formation, with a thickness of 3,800 feet, consists of bedded limestones, dolomites and calcareous shales, variable in detail along strike. It is abundantly fossiliferous, with a fauna of which stromatoporoids, corals, and brachiopods are the most important elements. A few genera and species are sufficient to characterize the formation in the field and include:

Gypidula sp. B[2]
Camarotoechia sp. A
Atrypa sp. D
Warrenella sp.
Paracyclas robusta Tolmachoff

Overlying the Blue Fiord Formation is a variable sequence, at least 2,950 feet in thickness, of limestones, sandy limestones and shales, and sandstones, named the Bird Fiord Formation. Fossils are reasonably abundant throughout and consist largely of corals, brachiopods, and trilobites. The most characteristic forms include:

"*Camarotoechia princeps* Barrande" of Meyer
Emanuella sp.
Cranaena sp. A
Dechenella sp.

The contact between the Bird Fiord and Blue Fiord Formations was chosen at a lithologic break marked by the first appearance of abundant quartz sand in the succession, and was found to correspond with an equally marked change in the faunas.

The highest formation, named the Okse Bay, consists of over 10,000 feet of largely non-marine quartzose sandstones and sandy mudstones and shales with thin coals. The contact with the underlying Bird Fiord is transitional, and drawn where calcareous sandstones with marine fossils give way to non-marine deposits with plant remains.

Although this paper is concerned with the definition of rock units, it might be of interest to record that the age of the Okse Bay formation, as evidenced

[1] The formation names used in this paper are provisional, and will be fully defined in forthcoming reports to be published by the Geological Survey of Canada.
[2] Letters of the alphabet are assigned as trivial names to easily recognized morphological species.

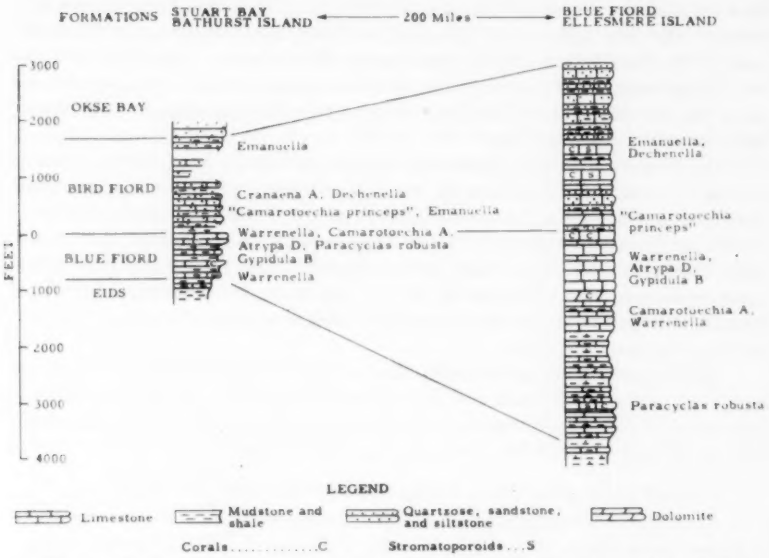

Fig. 2. Stratigraphic sections showing the development of Devonian rocks on Bathurst and Ellesmere Islands.

by plant and fish remains, is Upper Devonian, while the faunas of the Bird Fiord and Blue Fiord Formations are interpreted as being of Middle Devonian age. The Eids, on more indirect evidence, is considered Lower or Middle Devonian.

As mapping was continued to the southwest and west, the thickness and lithology of the marine Devonian rocks were found to vary considerably. It proved posible, however, to employ the same formations all along the fold belt as far as Bathurst Island, providing a sufficiently broad definition and interpretation was adopted for each. For instance, in the Stuart Bay area of Bathurst Island, the same four formations were mapped (fig. 2). The Eids is of similar lithology to that of the type area and about 1,000 feet thick. Beds assigned to the overlying Blue Fiord Formation vary between 600 and 770 feet of bioclastic limestones and black and grey calcareous shales. The fauna is similar to that of the type area; many species are common to both areas including those already listed. The Bird Fiord Formation is 1620 feet thick. It consists of calcareous and argillaceous quartzose sandstones and sandy limestones with intervals of calcareous and sandy mudstones which seldom outcrop; there are many covered intervals. The highly characteristic fauna is similar to that of Southern Ellesmere Island, including the forms named previously. Only a few hundred feet of the overlying Okse Bay Formation are present in the cores of synclines, within the area.

These formations were similarly recognized and examined at several intervening localities and elsewhere on Bathurst Island. The two main criteria

used to draw the boundary between the Bird Fiord and Blue Fiord Formations are (1) the first appearance upwards of quartz sand in any quantity at the base of the Bird Fiord and (2) the obvious differences in faunas between the two formations. Other criteria that proved useful in areas where outcrop was good include change in weathering characteristics, and increase in mud deposition upwards at the junction.

On Eastern Grinnell Peninsula, where outcrop is poor, faunal change proved the most reliable and easily mapped criterion in interpreting individual outcrops, especially the non-sandy limestones encountered at certain horizons within the Bird Fiord. Had the faunal change not been recognized in the field, these formations could not have been extended across the islands from Ellesmere westwards with the same degree of confidence. It is doubtful if the Bird Fiord-Blue Fiord boundary on Grinnell Penisula could have been mapped without much greater difficulty.

Subsequent interpretation of the faunas leads to the conclusion that in the type area of the Bird Fiord and Blue Fiord Formations, two biostratigraphic units may be defined, corresponding to these formations. These units will probably prove to be stages when the fauna is studied in greater detail.

RABBIT LAKE STRUCTURE, UPPER MACKENZIE RIVER VALLEY, N.W.T.

About 10 to 15 miles south of and paralleling Mackenzie River near where it drains from Great Slave Lake, a scarp of almost flat-lying Devonian limestone extends from Kakisa Lake westwards to beyond Trout River and thence, southwestwards to Blackstone River, a few miles upstream from the Liard (fig. 3). The scarp-forming limestone is part of a sequence of Upper Devonian rocks which is exposed, in part, along the Hay, Kakisa, Bouvier, Redknife,

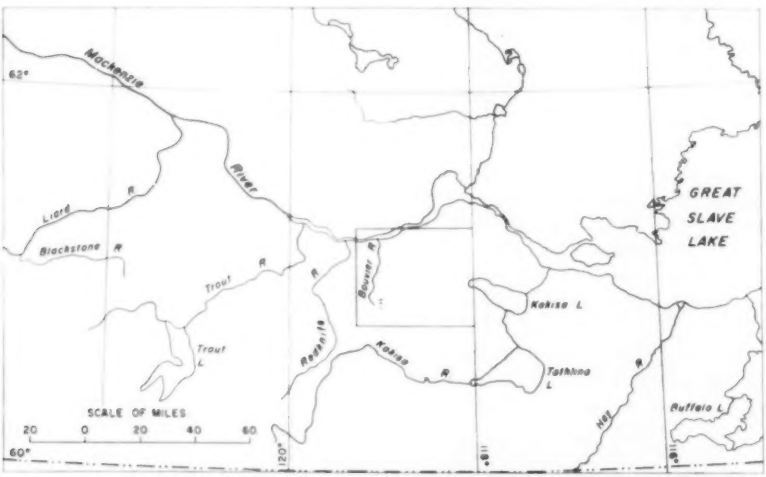

Fig. 3. Index map of Upper Mackenzie River, Northwest Territories. Smaller square indicates area covered by map in fig. 5.

Trout, and Jean-Marie Rivers. (The sections on these rivers have been described by Cameron, 1922, and Whittaker, 1922, and 1923, and more recently the Hay and Trout Rivers sections were summarized by Crickmay, 1957).

The scarp-forming limestone, here referred to the "Upper Grumbler" Formation,[3] forms a small canyon in most of the rivers mentioned above, but outcrops of the overlying Trout River and underlying "Middle Grumbler" Formations are poor. The exposure, nearest to the Rabbit Lake area, of a virtually complete section, is on Trout River, some 50 miles to the west (fig. 4). Here the "Middle Grumbler" consists of shales, sandy shales and shaly sandstones, all more or less calcareous. Over 100 feet are exposed. The "Upper Grumbler" grades upwards and is formed largely of well bedded, slightly sandy and silty limestones with some strongly bioclastic beds. Corals and stromatoporoids increase in abundance upwards, and, near the top of this formation, form

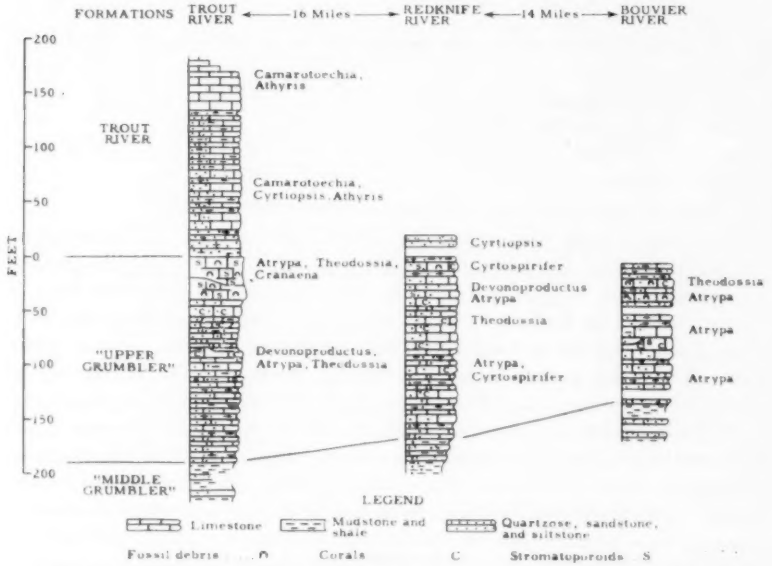

Fig. 4. Stratigraphic sections showing the development of Devonian rocks in the Upper Mackenzie River area.

bioherms up to 30 feet in thickness that are well exposed along the sides of the river. Depending on the thickness of these bioherms, 180 to 200 feet of "Upper Grumbler" are exposed along Trout River.

[3] The Grumbler Formation was defined on Hay River by Crickmay (1957), and a supplementary section cited on Trout River. Pending publication of final results of field work undertaken by the Geological Survey of Canada in 1957, the name is used informally away from the type area. The overlying Trout River Formation was defined at the same time by Crickmay, with type section on Trout River. The name is used in this paper for all the Devonian rocks in the area that lie above the Grumbler Formation. Preliminary maps and report on the area have been published by the Geological Survey (Douglas, 1959).

The boundary between the Trout River and "Upper Grumbler" Formations is sharp, being the change from coral-stromatoporoid beds below to calcareous sandstones above. Higher beds of the Trout River, however, bear more resemblance to the non coral-bearing beds of the "Upper Grumbler" and might easily be confused in isolated outcrop. Corresponding to the change in lithology there is a sharp and complete faunal break between the two formations (Crickmay 1957, p. 2). This faunal change has wide implications discussed more fully below, but it is very easily recognized in the field, as fossils are reasonably common in both formations. The "Upper Grumbler" is typified by:

stromatoporids
corals
Devonoproductus
Hypothyridina
Atrypa
Theodossia

None of these forms extends beyond the top of the formation. The Trout River is typified by:

Camarotoechia
distinctive rhynchonellids
Cyrtiopsis
Athyris

None of these forms occurs in the underlying formation.

Eastwards from Trout River the junction between the Trout River and "Upper Grumbler" Formations is seldom exposed. In addition, bioherms and coral beds in the "Upper Grumbler" become rarer and the formation thins. It may be studied on Redknife and Bouvier Rivers, but only on the Redknife is there a small outcrop of basal Trout River sandstone. Between Bouvier River and Kakisa Lake, aerial photographs reveal a structure involving doming up of the scarp-forming Upper Grumbler crossed by what appears to be a northeast trending fault (fig. 5). Outcrops are poor except for small areas of flat pavement that reveal at the most a foot or two of beds; nowhere are the basal sandstones of the Trout River Formation exposed. Nearly all outcrops were visited systematically by helicopter and, almost without exception, fossils were collected from each of them. On cursory examination in the field, each outcrop was assigned to its formation and, with the assistance of aerial photographs, the structure mapped.

There is little doubt that greater familiarity with the local characteristics of each formation, and longer time spent on each outcrop, would have achieved the same result without the use of fossils, but they are unquestionably the most easily recognized physical characteristic of the rocks concerned under these conditions of outcrop. In addition, all observations in the region did not have to be made by one geologist, resulting in a valuable saving in time under conditions necessitated by rapid reconnaissance mapping. Most of the exposures in the vicinity of the Rabbit Lake structure were visited by D. F. Stott, of the Geological Survey of Canada whose contribution is gratefully acknowledged.

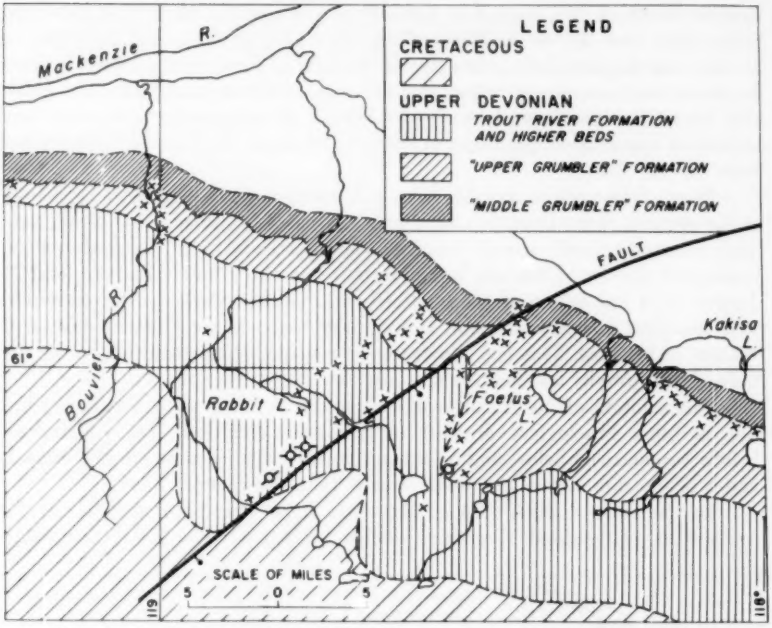

Fig. 5. Geologic map of the Rabbit Lake structure, Upper Mackenzie River.

BOUNDARY BETWEEN ALEXO AND MOUNT HAWK FORMATIONS IN ALBERTA ROCKY MOUNTAINS, EAST OF JASPER

The stratigraphy of the Upper Devonian rocks of the Alberta Rocky Mountains south of Athabasca River has been discussed by many workers (see Fox 1951 and McLaren, 1953 for historical summaries, and Belyea, 1958 for more recent work which includes the subsurface to the east). McLaren (1956) and Belyea and McLaren (1957) have developed a system of rock-stratigraphic classification based on earlier work. Only one aspect of this classification will be considered here.

The Fairholme Group is the lowest major unit in the sequence and is divided into several formations and members, vertically and laterally, in an attempt to formalize the complex interrelations of various facies bodies within the group. The Mount Hawk is the highest formation in the group and consists of a variable sequence of calcareous mudstones, argillaceous limestones, limestones and dolomites that change laterally into an all-carbonate sequence known as the Southesk Formation. Both formations are overlain by the Alexo and that in turn by the Palliser, a thick limestone formation in which there is relatively little lateral variation.

The Alexo Formation was defined (in summary) as a highly variable sequence of bedded and brecciated limestones, sandstones, dolomites, and shales, all containing and interbedded with quartzitic silt. Where this formation over-

lies the Southesk, the contact is normally sharp and the Alexo thin. Over the more shaly and thinner sections of the Mount Hawk, however, the Alexo is thicker and the boundary is less clearly marked. In some sections there is room for more than one interpretation, and the claim has been made that parts of the two formations interfinger laterally. As defined, the appearance upwards in any section of appreciable quantities of silt or sand might be taken as a convenient base for the Alexo.

Most of the work to date has been of a reconnaissance nature, and although many sections of the Devonian have been examined in considerable detail, they were relatively widely spaced geographically. Recently E. W. Mountjoy of the Geological Survey of Canada began mapping in the Miette map-area, east of Jasper, on a one-mile scale (fig. 6). During his field work, he has measured and described a large number of Devonian sections along the Miette and other Ranges in his area. This has allowed more reliable conclusions concerning details of stratigraphy and definition of map units.

The solution of the Alexo problem appears to lie in using fossils in addition to other lithologic characters in formation definition. The conclusions briefly summarized below and on the accompanying diagrams are provisional, and it is hoped to discuss the problem more fully elsewhere.

In the Front Range, west of Mountain Park (fig. 7), a section, on the north flank of Cardinal Mountain, yielded a fauna (Fauna X) from a shaly

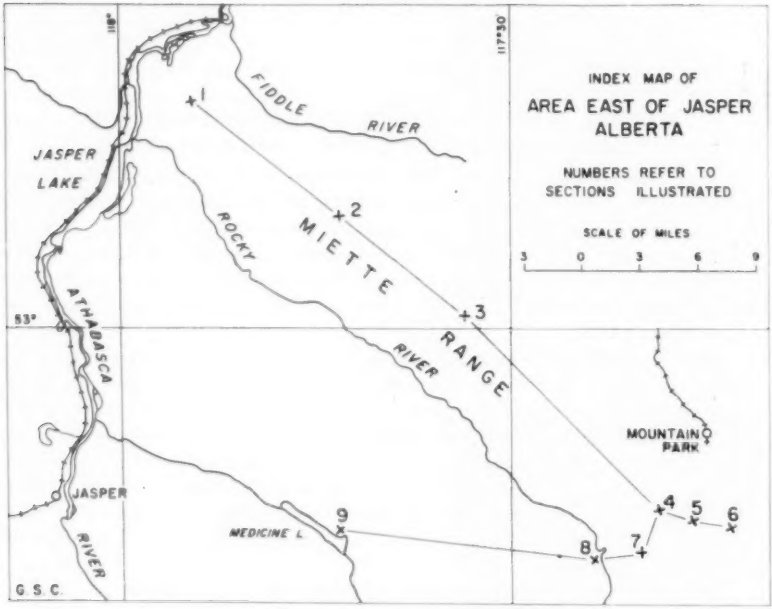

Fig. 6. Index map of area east of Jasper, Alberta. Numbers refer to stratigraphic sections summarily illustrated on figs. 7 and 8.

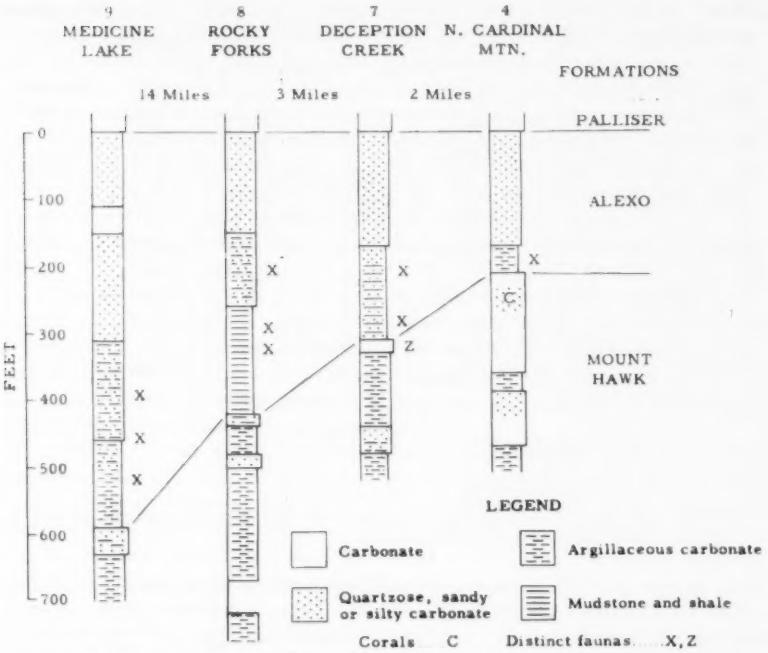

Fig. 7. Summary stratigraphic sections of Devonian rocks in area east of Jasper, Alberta. Location of sections is shown on fig. 6.

limestone in the lower part of what was considered on lithologic grounds to be the Alexo Formation, which is here 203 feet in thickness (Section 4). A similar fauna was collected at Deception Creek (Section 7, fig. 7), also near the base of the Alexo, here 292 feet thick, and at Rocky Forks (Section 8) in an Alexo 422 feet thick. Yet farther west, at Medicine Lake (Section 9), a comparable fauna was found near the base of an Alexo 601 feet in thickness.

Along the same range in which Section 4 occurs, the Alexo remains fairly thin and is elsewhere unfossiliferous (fig. 8). Within 1½ miles of Section 4, however, on the northeast flank of Cardinal Mountain (Section 5), the highest beds of the Mount Hawk contain corals typical of a fauna well-known elsewhere (Fauna Y). Northwestwards the same fauna, in fuller development was found at the head of Fiddle River (Section 3) in a similar position.[4]

At this stage interpretation of the faunas must be invoked in order properly to evaluate the relationships of the rock units concerned. Fauna X is identical with the fauna mentioned previously from the Trout River Formation in the Upper Mackenzie region. Fauna Y is closely similar, with some elements identical, to the fauna of the "Upper Grumbler" of the same region. The break between these two faunas sees the extinction of *Devonoproductus*, *Hypothyri-*

[4] Sections 2 and 3 were measured and described by Mountjoy, who also supplied further details on Section 1. This information is gratefully acknowledged.

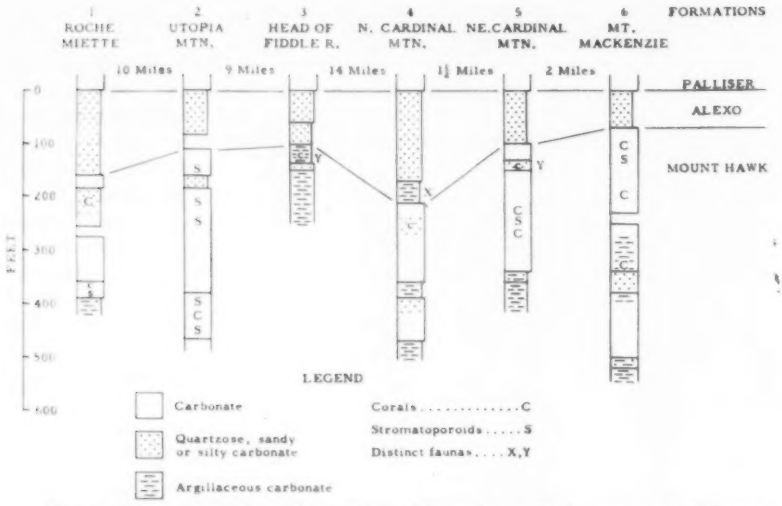

Fig. 8. Summary stratigraphic sections of Devonian rocks in area east of Jasper, Alberta. Location of sections is shown on fig. 6.

dina, *Atrypa* and *Theodossia*, and the disappearance of stromatoporoids and corals for the remainder of the Devonian. Above, many new species appear, new genera of productellids, the first *Cyrtiopsis*, and a spinose "*Athyris*", together with distinctive forms of rhynchonellids. The upper fauna is known from Nevada to the Mackenzie River and always overlies faunas similar or equivalent to the lower, or yet earlier assemblages. The two faunas are not known to overlap and, in fact, the break between them is one of the most significant faunal breaks in the Devonian of North America and Europe, where it defines the boundary between the Frasnian and Famennian Stages. This interpretation may now be applied to the rock stratigraphy.

It has been suggested that the explanation of the areal facies and thickness changes in the Mount Hawk and Alexo is due to partial lateral intertonguing of the two formations. Where the Alexo is thick and overlies a thin Mount Hawk at, for example, Medicine Lake (Section 9), the lower shaly and sandy beds of the Alexo are assumed to have been deposited at the same time and to interfinger with the upper beds of a thicker Mount Hawk elsewhere, e.g. at Roche Miette (Section 1) where it is overlain by a thin Alexo (Taylor, 1957, fig. 2). The faunal evidence, summarized on figures 7 and 8, denies this interpretation, and we must accept the conclusion that the Alexo overlies the Mount Hawk and was deposited subsequently to the highest beds of that formation.

Figure 8 shows various silt and sand horizons within the Mount Hawk. In a number of instances their deposition appears to have been inhibited by excessive carbonate development—a reef or carbonate bank (e.g. Section 2). The lowest horizon at which silt occurs has little significance and plainly varies

areally. A constant physical characteristic is, however, provided by fossils. Essentially, the presence of stromatoporoids and corals serves to unify the upper beds of the Mount Hawk and becomes a reliable guide to drawing its upper boundary. In spite of the lack of fossils in most exposures of the Alexo in areas where it is thin, lithological equivalence is easily established and non-fossiliferous sections tied in without difficulty. Key fossiliferous sections, such as that north of Cardinal Mountain (Section 4), furnish the basis for defining the rock unit boundaries along a whole range. Most sections (including many examined by Mountjoy) yielded corals, or fragments of corals and stromatoporoids, in carbonate beds high in the Mount Hawk, which assisted in establishing its upper boundary.

The Mount Hawk-Alexo boundary so defined becomes not merely a horizon that can be used for mapping, but also a geological boundary of great significance to the depositional history of the region. It should be mentioned in passing that the criteria used to define this boundary are also applicable in the subsurface; coral and stromatoporoid fragments are recognizable in well cuttings, and can provide confirmatory evidence.

This is not the place to discuss further the meaning of this formation boundary, but it should be noted that there is possibly some evidence of local non-deposition or even erosion prior to the deposition of the Alexo as now defined. This is provided by the presence of Fauna Z in Section 7, and in a similar position elsewhere. This fauna is believed to have existed earlier than Fauna Y and commonly occurs below it.

The revised definition and interpretation of the Alexo Formation is discussed here merely as an example of the value and importance of fossils in defining formations. This interpretation requires that some of the sections discussed by McLaren (1956) be revised. This will have little bearing on the question of major intertonguing *versus* superposition for the higher beds of the Mount Hawk and lower part of the Alexo.

CONCLUSION

The examples given above are largely self-explanatory. As with all simplifications, they make the problems seem much more cut and dried, more amenable to analysis, than they are in practice. Although I subscribe to the idea that formations are defined objectively, in fact the approach of the individual making any observations must unquestionably play a part. Perhaps I am guilty of special pleading in arguing for the use of fossils in the cases here outlined.

I would like to close on a more philosophical note. All Geology depends on one basic principle—that it is possible to interpret the history of the earth by examining the positional relationships of rock and mineral bodies. Stratigraphy relies primarily on a special case of this principle—the positional relationships of stratified rock bodies, from which derives the law of superposition. Stratigraphy can go a long way on this alone, for the law of superposition, although empirical is nevertheless incontrovertible.

William Smith enunciated another principle of stratigraphy—that strata can be identified by organized fossils. This is of considerable interest, because,

at the time at which Smith worked, there was no well-established logical relationship between his conclusion and the law of superposition, which he nevertheless accepted as fact.

Following the demonstration, very much later, of the fact of evolution, it became possible to derive Smith's principle by purely deductive reasoning. Given as premises (1) the law of superposition and (2) the fact of organic evolution (by whatever mechanism), then it must follow inevitably that fossils may be used to recognize strata.

This is not the sense in which I have been talking about fossils in most of this paper, but I do feel it worthwhile stressing the importance of the conclusion at a time when fossils are playing an increasingly minor role in many stratigraphic studies. However imperfect, and however difficult to apply, Smith's principle still stands as the only logically vindicated method of unravelling the history of the earth, in addition to conclusions drawn purely from positional relationships of rock bodies. To ignore this principle is surely to work with one hand tied behind one's back.

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[American Journal of Science, Vol. 257, December 1959, P. 752-769]

TECHNIQUES OF MOLLUSC ZONATION IN TEXAS CRETACEOUS

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ABSTRACT. The objective of the investigation, the kinds and numbers of fossils available, and the scale of the investigation all influence the type of zonation that will be used in any particular sequence of rocks. In an Edwards limestone study, the purpose being paleoecologic, the zones are based on sessile molluscs and are biosomal, but a single lithosome may be coextensive with more than one zone. No zones are coextensive with more than one lithosome; zonation is a technique with which to obtain data for further interpretation.

A study in the Georgetown limestone consists of a detailed correlation of appearances and disappearances of mutually overlapping ammonite teilzones. In this study the zones are those parts of teilzones that are or are not overlapped, arbitrarily depending on whether an appearance or a disappearance is more useful for the investigation.

Strata which can be correlated with the Austin chalk at Austin change rapidly from shale to chalk to limestone to black shale to flaggy limestone. The shale and limestone lithosomes contain ammonite sequences which are usually mutually exclusive. Within a single lithosome assemblage zones are useful and practical, but correlations from one lithosome to another is made only by the rare occurences of single ammonites which seem to have been buried outside of their optimum environment. These single occurrences are neither index fossils nor guide fossils in the most recent interpretations.

None of the four techniques used above produce units that can be called zones in a chronostratigraphic sense. All of them are zones in the wider interpretation of that word, and the Edwards limestone zones also approach the meaning of the term zone as used by some ecologists.

INTRODUCTION

The objective of the investigation, the kinds and numbers of fossils available, and the scale of the investigation all influence the type of zonation that will be used in any particular sequence of rocks. If paleoecology is the objective of a particular study, then one will likely use benthonic animals that are tightly tied to the bottom and to a narrow depth, salinity, energy, or other range (e.g., stenohaline, stenobathic, etc., forms). On the other hand, the usual technique for long range interregional correlations is the use of planktonic or nektonic assemblages.

If one is interested in more detailed regional or even local work to emphasize local geological history, then more refined methods of zonation must be applied. These might include the use of single teilzones, or, preferably, the use of homotaxial arrangements of parts of a succession of overlapping teilzones.

If correlating between rocks that represent extremely different environments, it may be necessary to extrapolate from the rare accidental occurrence of a nektonic species that for some reason was buried outside of its optimum environment. Ammonites have long been considered by some as occurring outside of their optimum environment by the floating of conchs, but Reyment's (1958a) recent paper puts a different light on this subject, and agrees more closely with Spath's (1933) earlier speculations. Reyment's work indicates that conchs with the shape of *Nautilus pompilius* Linné could float for great distances, but that conchs that depart from this shape only as little as *N. umbilicatus* Lamarck would sink within only a few miles. Disk-shaped forms

and extremely tumid forms likewise, according to Reyment, would not float any great distance. It requires only a glance at any introductory paleontology text to observe that a shape of *N. pompilius* Linné is extremely rare among ammonites.

Four different techniques of mollusc zonation that have been applied with different degrees of success to rock sequences in the Cretaceous of Texas are as follows:

1. biosomal zonation
2. zonation of overlapping teilzones
3. assemblage zones
4. correlation by rare accidental occurrences.

In the following pages I will try to show that these different techniques meet the different demands of diverse investigations and the abundance or rarity of fossils in separate sequences of rocks.

BIOSOMAL ZONATION

Purpose.—The Edwards limestone of southern North-Texas consists largely of a persistent and resistant limestone unit, from 20 to 30 feet thick, containing fossil pachyodonts (rudistids and caprinids). To the west-southwest of Blum (fig. 1), on the south side of the Brazos River in Bosque and Johnson Counties, the Edwards limestone has been removed by erosion, except for scattered and incomplete outliers. To the east-north-east of Blum the Edwards interfingers with the Goodland limestone. It is overlain disconformably by the Kiamichi formation and underlain conformably by the Comanche Peak limestone.

The purpose and result of the particular study was paleoecological. Although the investigation was cursory, some of the questions to be answered were depth, salinity, energy, etc., of the water.

Technique.—Since the available fossils are pachyodonts, and since these were sessile molluscs, they are used for the zonation. The normal process of measuring detailed sections and identifying the fossils by position in the section is followed. The taxonomic problem is not great because the different zones turn out to be identifiable by genera, rather than species. There is a problem on how to draw the boundaries between the zones. The boundaries between the *Gryphaea* and *Cladophyllia* zones and between the *Cladophyllia* and *Monopleura-Toucasia* zones (fig. 2) are sharp and distinct. On the other hand the remaining zones are transitional from one to the next. The top of the *Monopleura-Toucasia* zone is drawn at that horizon at which *Caprinuloidea* becomes a more dominant fossil than the combination of *Monopleura* and *Toucasia*. This is a subjective choice, and the margin of error is probably plus or minus one and a half feet. In the same manner the top of the *Caprinuloidea* zone is placed at that horizon at which *Eoradiolites* becomes more abundant than *Caprinuloidea*. This boundary probably has an error of plus or minus two feet.

Terminology.—The use of zone for this particular technique is in the sense of widest latitude that can be given to the meaning of that term. According to Teichert (1958) a biostratigraphic zone should be restricted to world-

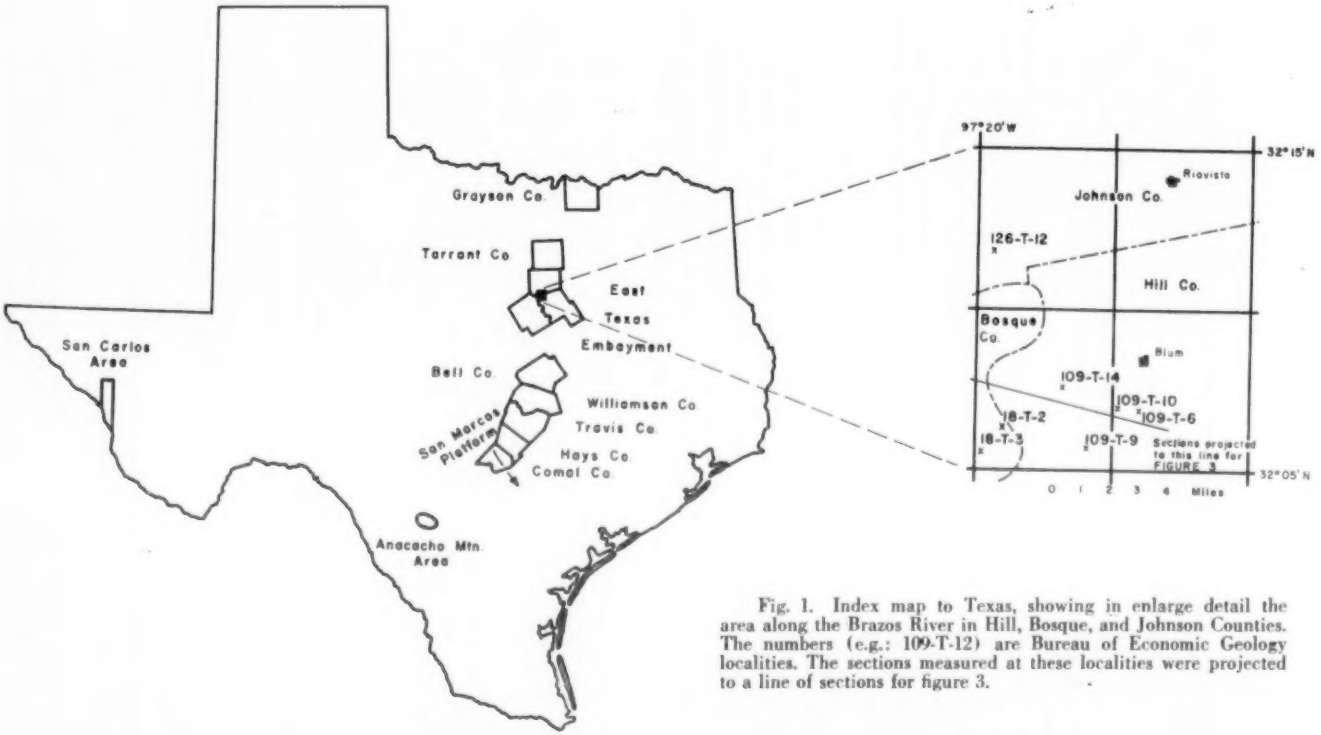


Fig. 1. Index map to Texas, showing in enlarge detail the area along the Brazos River in Hill, Bosque, and Johnson Counties. The numbers (e.g.: 109-T-12) are Bureau of Economic Geology localities. The sections measured at these localities were projected to a line of sections for figure 3.

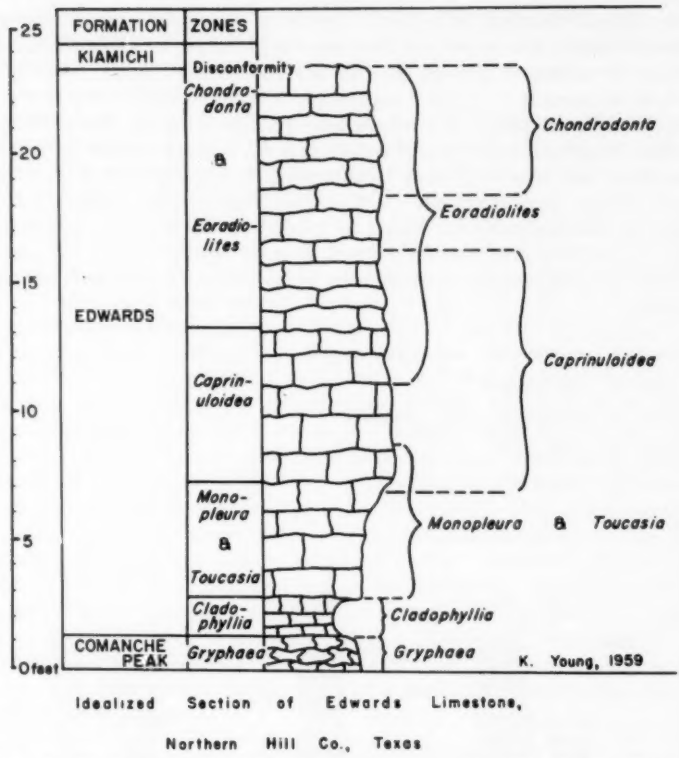


Fig. 2. Idealized section of Edwards limestone, northern Hill County, Texas. The brackets on the right indicate the local range of the zone fossils; these ranges do not apply farther west, farther east, or to the south. Figure after Young (1959).

wide assemblages of fossils of which the chronologic significance can be empirically demonstrated. The use of zone in the present technique is obviously not the biostratigraphic zone of Teichert's interpretation. The pachyodont zones used here are almost equivalent to the biosomes of Wheeler (1958), except that in order to diagram them (fig. 3) it was necessary to simplify them. They are not, conceptually, the same as Wheeler's (1958) zones, because to me, if the fauna is recurrent, the zone is recurrent, and the arbitrary cut-off does not have to be used in biostratigraphy. The recurrence of a zone does not, automatically, presuppose faulting, or folding, as does the recurrence of a formation on a map.

I prefer, then, to keep the meaning of the word zone at its widest latitude, as does Hedberg (1958). If a term is really needed for the biostratigraphic zone of Teichert (1958)—if geochronology has advanced sufficiently that such a term is useful—then one should be proposed, the meaning of which is still

definite. Of course, there is always the question of whether to restrict a term to its original meaning, or to coin a new term. Generally terms, if allowed to evolve normally, will grow and their meaning change, as ideas change, even without the misuse of persons who do not know their original meaning. Restriction of meaning to original definitions, if followed rigidly, would not only stabilize the terminology, but would also stabilize thinking. Since this is impossible, restriction really would result in an even larger number of terms because every new idea could only be expressed by the definition of a new term. Arkell (1956) and Hedgpeth (1957a) also discuss this problem. In other words the biostratigraphical language needs a word with the meaning that zone has in its widest latitude. I think it more feasible to use the one that evolved to that meaning rather than to try to propose a new term of general meaning.

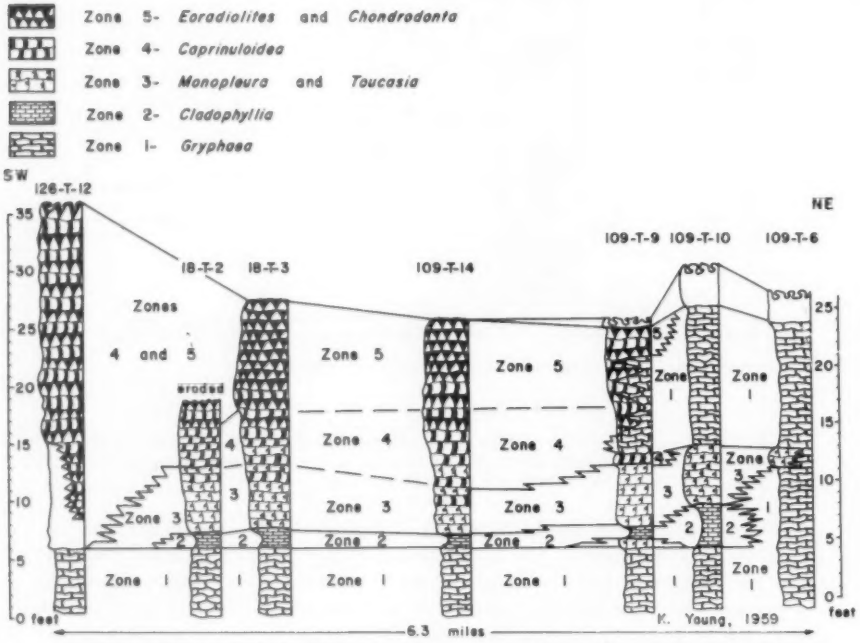


Fig. 3. Zonation of the Edwards limestone in Hill and Bosque Counties, Texas, modified after Young (1959). The patterned parts of the diagrammatic section denote areas in which sessile mollousks are still in growth positions. The different zones depict environments of different energies. The numbers at the tops of the sections are Bureau of Economic Geology locality numbers.

Further validation of this use for the pachyodont units of the Edwards limestone is that these zones represent the biologic units called zones by some ecologists (Hedgpeth in Ricketts and Calvin, 1952; Doty, 1957; Hedgpeth, 1957b; Wells, 1957), although to Thorson (1957) the zone appears to be the

interpretation applied to the community—the sublittoral zone, etc. For a classification similar to the Edwards limestone zonation described here, based on energy of the environment, Lowenstam (1957) uses stage, which seems even less adequate than zone.

Generally paleontologists have not used zone for ecological units, probably because of the prevailing association of a time concept with the biostratigraphic use of zone, and also because fossil zones, in the ecologic sense, are usually extremely difficult to determine.

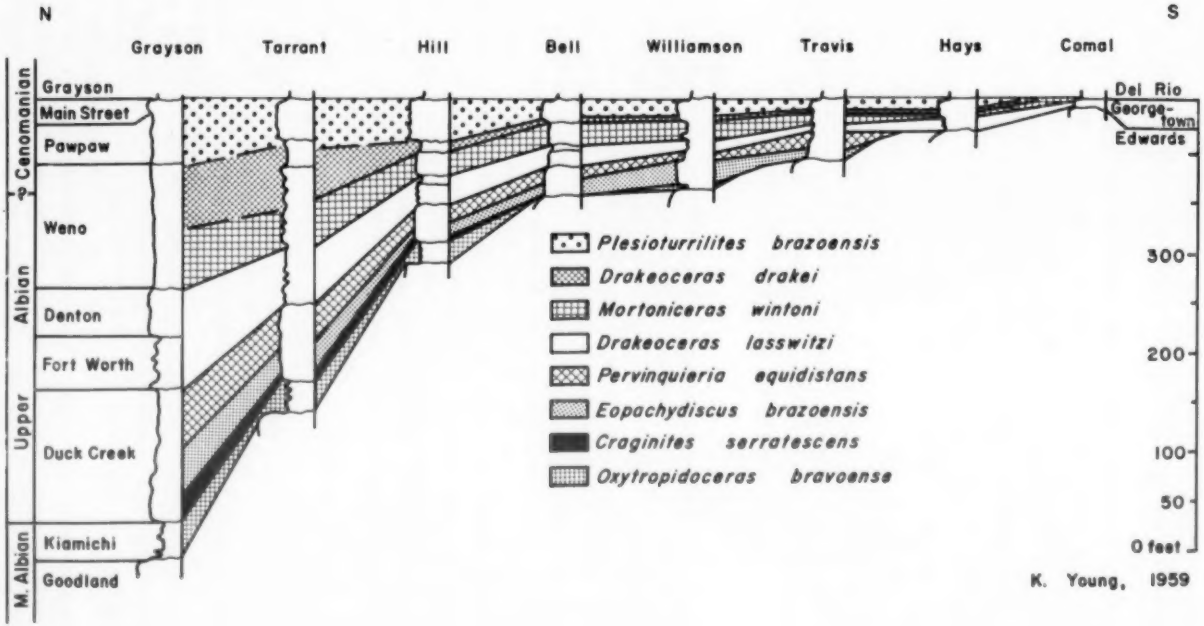
Results.—The results of the zonation have been published elsewhere (Young, 1959). The *Gryphaea* zone is restricted to the white nodular limestone of the Comanche Peak and Goodland formations. The *Cladophyllia* zone, the zone of a small ramose colonial scleractinian coral, and the only non-mollusk in the classification, is restricted to a pulverulitic bed overlying the Comanche Peak limestone.

The *Monopleura-Toucasia*, *Carpinuloidea*, and *Eoradiolites-Chondrodonta* zones make up the typical reef rock of Nelson (1959), and these are the dominant reef-building fossils that can now be identified. They compose one lithosome, but if one is familiar with the different patterns which the sections of the fossils give to the exposed surface of the rock, fossil identification is not difficult (Bonet, 1952, especially figs. 10, 11, 14, 17, 19). A petrographer could obtain about the same zonation from carbonate lithology, except hand specimen analyses would also be necessary because the fossils are too large for the identification of dominant species in thin section. In other words the zones reflect the environment of deposition so well that each zone or group of zones is coincident with a rock type. This is the reason I have labeled this "biosomal zonation", stealing a term from Wheeler (1958) without his permission. The zones are the counterparts of deposition in zones as that term is used by some ecologists.

ZONATION BY OVERLAPPING TEILZONES

Purpose.—During the early part of a series of mapping projects along the Balcones Fault Zone it became desirable to understand the detailed stratagraphy of the Georgetown limestone and its equivalent formations along their strike. Figure 4 is a cross-section which leads diagonally out of the western margin of the East Texas Embayment, in the vicinity of Tarrant County, southward by slightly west onto the San Marcos Platform (Lozo and Stricklin, 1956), Comal County, Texas (fig. 1). Mapping had already indicated that some beds were being cut out at the base of the Georgetown formation, and consequently it was considered that Cuyler's (1929) diagram could be improved upon. The purpose of this project, in short, was to learn as much as possible by biostratigraphic methods of the details of the stratigraphic and tectonic history of the west side of the East Texas Embayment during the deposition of the Georgetown limestone and its equivalent formations to the North.

Technique.—The technique begins as all good stratigraphic techniques must, with the detailed measuring of sections and the recording of the positions of all of the fossils that might serve a useful zonation purpose. Since the


N
S
Grayson
Tarrant
Hill
Bell
Williamson
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Hays
Comal
Upper Cenomanian
Albian
M. Albian
Grayson
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Pawpaw
Weno
Denton
Fort Worth
Duck Creek
Kiamichi
Goodland
Del Rio
George-town
Edwards
Plesioturrilites brazoensis
Drakeoceras drakei
Mortoniceras wintoni
Drakeoceras lasswitzi
Pervinquieria equidistans
Eopachydiscus brazoensis
Craginites serratescens
Oxytropidoceras bravoense
300
200
100
50
0 feet
K. Young, 1959

area is about 300 miles long, many sections were measured, and the most important selected for the illustrations. In addition to myself and the part time services of two assistants, some 15 graduate students have helped in building up the lithic and biostratigraphic grids. Kummel collected fossils in Hill and McLennan Counties, with excellent detail and accuracy; these are deposited in the Bureau of Economic Geology, and were very useful in this study. The collections of W. S. Adkins were also utilized, and that remarkable paper by Adkins and Winton (1920), "Paleontological Correlation of the Fredericksburg and Washita formations in north Texas", for which the stratigraphy and paleontology was done over 40 years ago, was done in such careful detail and with such accuracy that it is still useful for detailed studies such as the present one.

The fossils being recorded, it was then necessary to determine the teilzones for each section; an example is the section at Georgetown, Williamson County (fig. 5). The next step, if detail and refinement is desired, is to divide the sections into as many zones as desirable, or possible. The greatest number of zones can be obtained if the section is divided into segments arbitrarily selected on the appearances and disappearances of teilzones (fig. 6). Any zone is then named after the teilzone of which is is an effective segment. If further detail is desired the overlaps can also be recorded, but some other type of designation then becomes necessary because more units are being used than there are fossils. On the scale of the present diagrams it was impossible to record even all of the teilzones, and the teilzone of *Idiohamites fremonti* has been arbitrarily included in the three lower zones. It should be emphasized that unless fossils are extremely abundant this type of zonation is not possible.

Terminology.—Again the question of terminology arises. I have used teilzone in the manner of most American paleontologists, following the interpretation of Arkell (1933). Teichert (1958) points out that Arkell misinterpreted the original definition of teilzone, and that we should use, instead, Moore's (1957) topozone. If this is the desirable term, I have no objection to using topozone.

The zones resulting from the teilzones or topozones in the Washita group are not comparable to the biostratigraphic zones of Teichert (1958). They may be compared to Teichert's subzones, although I have not used them in any sense as subdivisions of zones. Perhaps they are so local that they should be called zonules, but again they in no way represent subdivisions of the ammonite zones that can be used for interregional correlation of the Upper Albian and Lower Cenomanian rocks. The zones here defined for a particular technique constitute a zonation devised for and applied to a specific problem.

Results.—The results of this study were presented (Young, 1956) at the

Fig. 4. Diagrammatic section illustrating a zonation of the Georgetown limestone and its North Texas equivalents. The extent of the diagram is from Grayson County on the north to Comal County on the south, paralleling approximately the western margin of the East Texas Embayment (fig. 1). The zones are based on overlapping homotaxial sequences of teilzones. All zones thin toward the San Marcos Platform (south-south-west), and each of the four lower zones is successively overlapped by the next younger zone. The line of section for figure 4 is from Grayson County through the counties illustrated on the map to Comal County on the southeast edge of the San Marcos Platform.

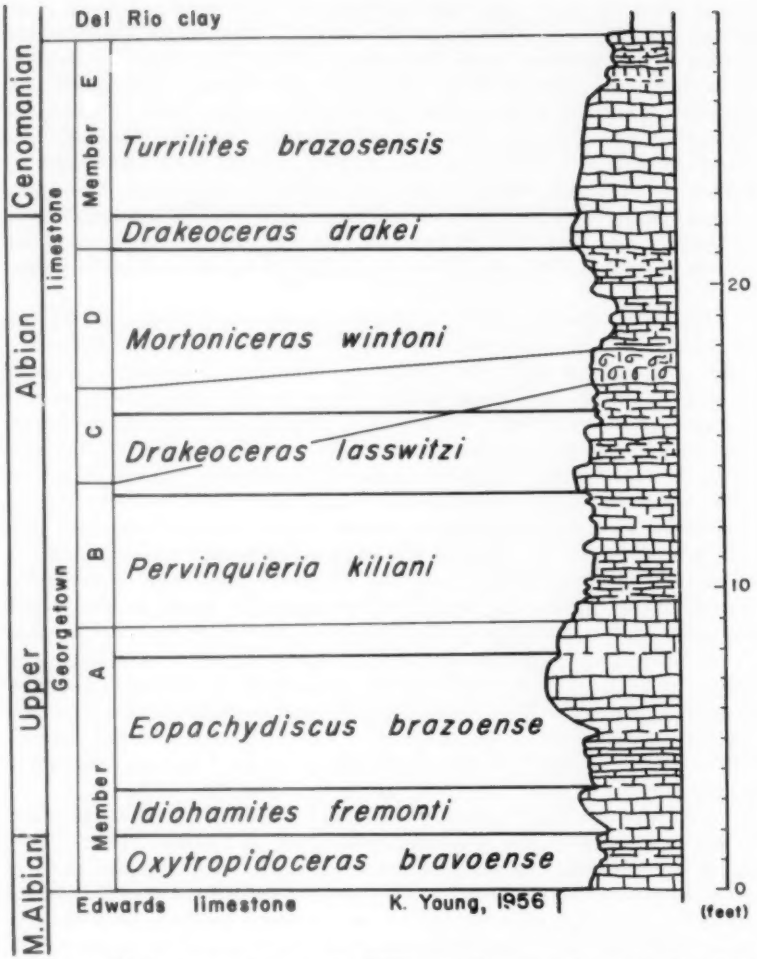
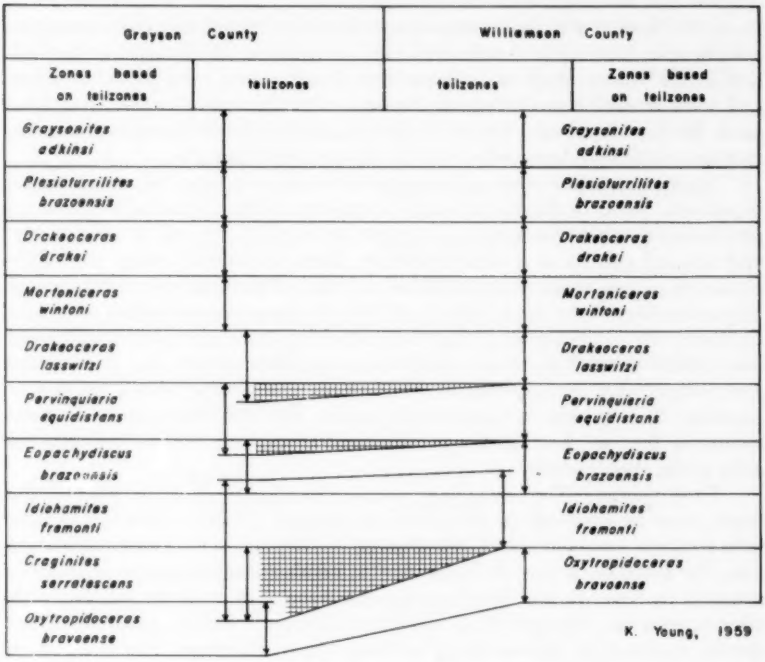


Fig. 5. Idealized control section for Williamson County; each zone is the teilzone for that species. It is only coincidence that the base of the *Drakeoceras drakei* zone is the same as the base of member E. There is otherwise no correspondence of zones and lithology.

20th International Congress, and should be published when the Cretaceous Symposium is eventually published. When the teilzones are redrafted as units, the diagram of the zonation of the Georgetown limestone and related formations appears as in figure 4. If the homotaxial sequence is consistent in all of the sections, there is something objective with which to correlate, although there is great room for argument as to its interpretation. In the Washita group



Comparison of Teilzones,

Grayson and Williamson Counties, Texas

Fig. 6. Diagram illustrating the relations of teilzones in Grayson County to the teilzones in Wililamson County. In the thin (100 ± feet) Williamson County section there is overlap of only the teilzones of *Idiohamites fremonti* and *Eopachydiscus brazoensis*. In the thicker (seven-fold) Grayson County section all of the lower teilzones overlap, but there is, as yet, no evidence of overlap of the younger teilzones. Shaded sections show biologic relations in Grayson County which are absent in Williamson County.

this gives a picture of all zones thinning from the basin to the platform, and of the lower zones being successively overlapped.

ASSEMBLAGE ZONES

Purpose.—The purpose of this particular investigation is to learn as much as possible about the geologic history and biostratigraphic correlation of the various parts of the Austin chalk. Unfortunately ammonites are not nearly so abundant in the Austin group as they are in the Washita group. Consequently, the less refined method of assemblage zones is used, and as a result, less is known concerning the Austin group.

Technique.—The technique does not differ greatly from other techniques to start with, because some form of a lithic grid must be established into which to arrange the fossils. This means that either detailed sections must be meas-

ured or that the fossils must be tied into some horizon such as the disconformity at the base of the Dessau limestone (Durham, 1956). Because ammonites are not abundant in the Austin chalk, the information which leads to the zonation of the Austin chalk has accumulated slowly over a long period of years, and were it not for such dedicated collectors as Clarence Durham, Roy Hazzard, W. S. Adkins, and Oscar Paulson, it is doubtful if enough Austin ammonites would have been collected to serve any useful purpose.

Some ammonites seem to have run in schools, at least they were buried in schools. Thus, in the Austin chalk it has been difficult to build up the assemblages, because one species of ammonite would be found at one locality, and another species at a second locality. Only occasionally were the representatives of two or more species found together so that the assemblages could be put together. Even then, were it not for the known superposition of genera in other parts of the world, so that the problem could be approached with some prejudice, I doubt if the results would be as complete as they are. Because the assemblages were put together in this fashion, and with a certain amount of intuition, they remain to some extent suspect, but they also are the best information that we have at the present time concerning the biostratigraphic units of the Austin chalk.

Terminology.—The assemblage zones of the Austin chalk, as used in this paper, may be subzones in the sense of Teichert (1958). Some elements of each zone are intercontinental, but whereas one element of the *Prionocycloceras* zone, for example, occurs in North Africa, another element occurs in Europe, and only in America are these two elements yet known to occur together with other species not reported from Africa or Europe. I cannot name all of the species at this time because many of them are undescribed; this is no place to introduce *nomina nuda*.

The assemblage zones I am using, if we believe in evolution, must be the result of past evolutionary processes, but the basis of their application is not evolution; it is that of empirical homotaxial arrangement. They are not based on phylogeny, because we do not know the phylogeny; at least there is no great agreement between Reyment (1958b), Wright (Arkell, Kummel and Wright, 1957), Matsumoto (1955), and Collingnon (1948). Spath (1934) points out that "if biological order does not agree with observed stratigraphic succession there can be no doubt that our views of what constitutes 'biological order' must be erroneous". The phylogenies of Reyment, Wright, Matsumoto, and Collignon do not agree because the stratigraphical successions of Upper Cretaceous fossils in the different parts of the world are different. In other words collection failure results in short range zones and a horizontal classification; the taxa as now defined are polyphletic. Obviously, if there is so much disagreement, the phylogenies are not sufficiently well known to base the zonation on them. Furthermore, most of the present ideas concerning the phylogeny of these animals is based on their order of superposition. The empirical body of data concerning the superposition and homotaxial arrangements of these assemblages of ammonites is the only basis for their validity. This may be orthochronology in the interpretation of Teichert (1958), but evolution is not explicitly the basis for these assemblage zones.

Results.—In figure 7 I have shown only the upper part of the Austin group, for convenience, and more detailed information will be published in a forthcoming paper. Already the age interpretation of the upper part of the Austin chalk has been changed as a result of this study (Young, 1958). The Austin can be divided into about 8 ammonite zones: the lower is dominated by certain species of *Peroniceras*, the second is dominated by *Gauthiericeras* and

Substage	Central Texas	Anacacho Mtns.	San Carlos Area
Middle Campanian	*Delawarella* cf. *delawarense*	*Delawarella* cf. *delawarense*	*Submortoniceras* n. sp. *Delawarella* cf. *delawarense*
Lower Campanian	*Submortoniceras* n. sp. aff. *tenuicostulatum*	?	*Submortoniceras* n. sp.
Lower Campanian?	*Bevahites* cf. *bevahensis*	*Bevahites* cf. *bevahensis*	*Bevahites* cf. *bevahensis*
Upper Santonian	*Texanites texanus gallicus*	*Texanites texanus gallicus*	
	Texanites texanus	?	
Lower Santonian	*Texanites stangeri densicostus*		

K. Young 1959

Assemblage Zones of Central Texas & Anacacho Mtn. Area, Compared with Accidental Indices of the San Carlos Area, Texas

Fig. 7. Texanite assemblage zones of Central Texas and the Anacacho Mountain Area, compared with accidental indices of the San Carlos Area, Texas. The two or three individuals each of *Bevahites* cf. *bevahensis* and *Delawarella* cf. *delawarensis*, coupled with the reverse occurrence of *Placenticeras guadalupae* (one occurrence, see fig. 9) are a tenous key to the correlation of the San Carlos beds with the Austin chalk, other criteria being absent.

Prionocycloceras, the third is the zone of *Texanites stangeri densicostus* (Spath), the fourth the zone of *Texanites texanus* (Römer), the fifth the zone of *Texanites texanus gallica* Collignon, the sixth the zone of *Bevahites* cfr. *B. bevahensis* Collignon, the seventh the zone of *Submortoniceras* sp. aff. *S. tenuicostulatum* Collignon, and the eighth is the zone of *Delawarella delawarensis* (Morton). These are ammonites that are the index species to the zones, but other species are just as indicative of the assemblage. Although

much more needs to be done, a reasonable zonation for the Austin chalk is being worked out, but it is not as refined as would be desirable for regional biostratigraphic purposes.

CORRELATION BY RARE ACCIDENTAL OCCURRENCES

The San Carlos area (fig. 1) is included in figure 7. The ammonites of the San Carlos beds were described by Hyatt (1903), and little has been done with them since. The fossils described by Hyatt were collected by R. T. Hill, T. W. Vaughan, and T. W. Stanton before 1900, and without the precision that is desirable in any more refined stratigraphic technique. There are about 5,000 ammonites now available from different horizons of the San Carlos beds. Nearly all of these are placenticerines or pseudoschloenbachiines. There are two assemblage zones, the upper one is the zone of *Placenticeras meeki*, with *P. meeki*, *P. guadalupae*, *P. sancarlosense*, *P. pseudosyrtale*, *P. planum*, *P. newberryi*, and *Pseudoschloenbachia chispaensis*. The lower zone, the *Placenticeras guadalupae* zone, contains approximately the same assemblage except for the absence of *P. meeki*; the first appearance of *P. meeki* arbitrarily marks the base of the Campanian, by definition, when placenticerines are used for zonation purposes. Unfortunately *P. meeki* is practically unknown from the carbonate deposits. Although, as I shall try to show, the San Carlos beds are equivalent at least to the upper part of the middle Austin chalk and of the upper Austin chalk, Adkins (1929; 1933) considered them to be of Taylor age. Although we do not know why he reached this conclusion, there are probably three reasons: (1) he accepted the concept of a regional disconformity at the top of the Austin chalk (Adkins, 1936); (2) the San Carlos beds contain no ammonite assemblages in common with the Austin chalk; and (3) the ammonite assemblages of the Taylor were and are still unknown. Consequently, the age designation of the San Carlos beds was based on "negative correlation". These beds contained no Austin chalk ammonites, so they must be of some other age; "negative correlation" is always a danger when dealing with pelagic assemblages of different environments.

In deference to Adkins I should point out that the top of the Austin chalk had been considered as Lower Santonian because of misidentification by paleontologists usually other than Adkins, misidentifications that were still being made as late as 1952 (Young in Young and Marks, 1952).

Purpose.—The purpose of the investigation, now in progress, is to determine the biostratigraphic relationships, if any, of the Austin chalk and the San Carlos beds.

Technique.—The technique cannot be applied until a large number of ammonites have been obtained from the rocks representing each of the two contrasting environments. This is because, as Newell (1959) has pointed out, the probability of finding a particular fossil increases with the number of different and distinct periods of searching for it. In other words, the correlation of the rocks representing the black shales of the San Carlos beds and the light colored limestones of the Austin chalk depends on the accidental discovery of a fossil which was buried outside of its optimum environment. Here are a few examples to illustrate just how tenuous such a correlation is.

The only specimen of *P. guadalupae* known from the Austin chalk is the holotype, collected by Römer about 1846 and described by him in 1852. I can assure you that the type locality has been visited many times by many of us, and much of the Austin chalk has been searched, but not one individual of this species has been collected, to my knowledge, in the ensuing 100 years. The lithology of Römer's individual verifies the locality he lists; furthermore, he could not have ventured far enough west to collect from a locality where many of these fossils do occur because of first, the Comanche Indians and second, the Apache Indians. What are the odds that Römer, who spent all of six weeks on the outcrop of the Austin chalk, most of it on horseback, would find the only individual of *P. guadalupae* ever to be found east of the Pecos River? And this is the holotype! Rare individuals of *P. pseudosyrtale* and *P. planum* have been recovered from the Austin chalk, but no *P. sancarlosense* and no *P. newberryi*. The isolated occurrences of *P. pseudosyrtale* and *P. planum* in the Austin chalk have not been tied into the stratigraphic grid and do not mean much. Furthermore, rocks cannot be zoned as well by placenticerines as by texanitines (figs. 7 and 8).

The Austin chalk, on the other hand, is dominated by various texanitines, pachydiscines, and puzosiines. So far no pachydiscines have been collected from the San Carlos beds, only one puzosiine, and a few scattered texanitines, yet 5,000 or more ammonites are in the collections from these strata.

This means that correlation between the two areas can be only by texanitines, but unfortunately the Austin chalk and the San Carlos beds do not always contain the same species, and where a species is present in each area, it seems to be represented by different geographic subspecies. We know so little of the relation between subspecies and species that this situation throws doubt on the validity of any correlation made with them. In other words, a splitter would describe my presumed San Carlos and Austin texanitine subspecies as different morphospecies; a lumper would describe them as one biospecies.

The real reason for correlating the strata of these two areas is the apparent accidental occurrence of individual texanitines out of their optimum environment. A correlation on these may eventually help in a decision as to whether the use of subspecies in the texanitines of this area is valid or not.

Moon (1953) has collected one individual each of *P. guadalupae* and *P. sancarlosense* from the Fizzle Flat member in the Agua Fria quadrangle of the Big Bend area. Moon does not mention the fossils, but they are in his collections at The University of Texas. The Fizzle Flat member contains the Lower Santonian texanitines, *Paratexanites*, n. sp. and *Texanites stangeri*, which indicate that *Placenticeras guadalupae* ranges down into the Lower Santonian to a horizon below *T. texanus texanus*. The *P. meeki* zone of the San Carlos beds has yielded one or two individuals of *Delawarella* sp. cfr. *D. delawarense* (Morton), plus the taxon that may prove to be a geographic subspecies of *D. delawarense*. *D.* sp. cfr. *D. delawarense* also occurs in the top of the Burditt marl (upper Austin chalk) below a Middle Campanian ammonite from the overlying Taylor, the latter related to *D. roedereri* Collignon. *D.* cfr. *delawarense* has also been collected from the Gober chalk. This is a tenuous correlation based on less than half a dozen individual fossils. In the San Carlos

Substage	Central Texas	Anacacho Mtns.	San Carlos Area
Middle Campanian		*Placenticeras meeki*	*Placenticeras meeki*
Lower Campanian	*Placenticeras pseudosyrtale*		
Lower Campanian?			
Upper Santonian	*Placenticeras guadalupae*		*Placenticeras guadalupae*
Lower Santonian			

K. Young 1959

Accidental Indices, Central Texas, & Anacacho Mtn. Area, Compared with Assemblage Zones of San Carlos Area, Texas

Fig. 8. Accidental indices, Central Texas and Anacacho Mountain Area, compared with assemblage zones of the San Carlos Area, Texas. In the right column are shown the two placenticerine assemblage zones of the San Carlos Area. The rocks of the other two areas are represented by rare finds of identifiable placenticerines, which have been dated by the texanitines with which they are associated. A correlation by placenticerines is impossible with the present data.

Area *Placenticeras guadalupae* ranges from Lower Santonian to Lower Campanian inclusive, if texanitines are used for zone fossils.

A new species of *Texanites* is found in the base of the Dessau formation with *Bevahites* sp. cfr. *B. bevahensis* Collignon. This most likely Upper Santonian species is also represented by 2 or 3 fragments from the *P. guadalupae* zone of the San Carlos area. One individual of the Santonian ? *Phlycticrioceras douvillei* (Grossouvre) is also known from the Austin chalk, from below the disconformity at the base of the Dessau, and another is known from below the *P. guadalupae* zone in the San Carlos area.

Results.—These few occurrences, then, indicate the equivalency of the San Carlos beds to the middle and upper Austin chalk, but in spite of all of the preceding discussion, they do not provide any refined method of correlation. These accidental occurrences of fossils deposited out of their optimum environment are not guide fossils or index fossils in any of the accepted definitions. I

have called this technique "correlation by rare accidental occurrences", but the technique is one that could, with other techniques, be included in the biozone or range-zone correlation of Hedberg (1958).

SUMMARY

In summary I will again emphasize that a theoretical system of zonation is not setup in the office, and then easily applied. Rather the fossils are collected, their geographic and stratigraphic occurrences recorded and studied, and a biostratigraphic grid (zonation) developed which best helps to solve the problem being investigated with the data available. If the zonation is to aid in ecologic studies one chooses benthonic fossils which most closely reflect the energy (or other) conditions of the environment. Such zones are biostratigraphic, but not chronostratigraphic. They probably represent the rocks deposited in a zone as that term is used by some ecologists; this is almost the biosome of Wheeler (1958).

If sufficient fossils are available then a refined system of zonation is possible, and it may be used in much the same way as subsurface geologists use the "key bed" technique. This involves tracing appearances, or disappearances, and naming the parts of the teilzones that best serve the purpose of the problem. This technique is only applicable in rocks in which particular zone fossils can be readily collected. It achieves validity only in homotaxial sequences when a body of empirical data indicates a high probability of finding the same homotaxial sequence the next time a section is measured or collected. The technique ceases to be valid when or where the homotaxial sequence breaks down. For this latter reason the technique is useful only locally.

Assemblage zones as a technique are unusually dangerous because most assemblages change as a result of an environmental change, and if the environment was transgressive, or regressive, as most environments were, so are the boundaries of the assemblage zones transgressive, or regressive. In the Austin chalk I have used assemblages of pelagic ammonites which are restricted to certain realms of the carbonate deposition. Assemblage zones are used because a better technique has not been discovered for this sequence of rocks.

In an attempt to correlate from the Austin chalk to the San Carlos beds, two sequences of strata which represent environments that were mutually exclusive of pelagic ammonites, even assemblage zones are impossible, and it is necessary to use only fossils that represent accidental burials outside of their normal environment. This is a technique of the last resort.

Again it should be emphasized that zonation techniques must be fitted to (1) the purpose of the investigation, (2) the degree of refinement or detail required by the investigation, and (3) the availability of the fossils that are to be used.

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[American Journal of Science, Vol. 257, December 1959, P. 770-778]

STRATIGRAPHIC CONCEPTS IN VERTEBRATE PALEONTOLOGY

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ABSTRACT. Vertebrate paleontologists use "local fauna" for the totality of taxa found at one or a few neighboring localities and prefix a geographic name; i.e., Garvin Gulley local fauna. If the fauna can be traced more or less continuously within a sedimentary province, the word "local" is dropped; i.e., Garvin Gulley fauna. This would be equivalent to the biostratigraphic term range zone.

Type sections in stratigraphic classification should have no more significance than name bearers. They are one dimensional samples of three dimensional bodies of rock.

A worker in vertebrate paleontology is sandwiched between geology and zoology. He is usually regarded as a biologist when speaking before geologists and therefore a person out of the field of the audience; as a geologist when talking before biologists, so again, tolerated but not to be taken seriously. But there are advantages; he gains from seeing how things are progressing in the two adjacent sciences, to his right and to his left. Speaking in a political sense I would place stratigraphy well to the right.

The stratigraphic concepts of which I wish to speak, I feel are somewhat leftish, that is toward, or perhaps better words would be, more analogous with, concepts held by some biologists. These concepts are not confined to vertebrate paleontology; some of my colleagues would deny them. They do, I am sure, play a role in the thinking, although I have not seen them so expressed in print, of some vertebrate and some invertebrate paleontologists. These ideas were arrived at, in part, over numerous cups of coffee with Drs. W. C. Bell and K. Young at The University of Texas. I am grateful for their stimulating help but assume full responsibility for this paper.

The continental sediments along the Texas Coastal Plain furnish a record of successive vertebrate faunas that very nicely illustrate certain points in stratigraphy. Vertebrate paleontologists have used the term "local fauna" to apply to the totality of species collected from one important site or cluster of sites. Thus the name Garvin Gulley local fauna was given to the vertebrates found on the Garvin farm just north of Navasota. Over a period of several years additional collections have been made. Now elements of the Garvin Gulley local fauna can be identified from localities (fig. 1) distributed over an area 250 miles long. Such a distribution is no longer "local" so the term Garvin Gulley fauna has been applied to the sum total of the local faunas.

The term is comparable to assemblage zone but instead of using a biologic name with all the vicissitudes to which it is subject, vertebrate paleontologists use a geographic name. Thus we have a biostratigraphic unit, the Garvin Gulley fauna, based on collections of vertebrate fossils similar to those at Garvin Gulley.

To the south is the Burkeville fauna similarly traceable over an area 300 miles long. Still farther south is a third fauna, named after the town of Cold Spring in San Jacinto County.

All three faunas have some vertebrates in common with one or both of

Fig. 1. Diagrammatic representation of distribution of fossil localities in the Miocene of the Texas Coastal Plain.

the others: turtles, gar fish. Each fauna shows evolutionary changes in some of the mammals from those of the preceding fauna: the horses, for example. Each fauna has some new forms introduced or old ones missing. It is this change in total composition that enables us to differentiate the faunas one from the other.

For purposes of convenience of expression I can draw lines separating each of these faunas (fig. 2). These lines can only be found on this diagram and on a couple of others in print. They cannot be found in the field anymore than contour lines can be found in the field. Each collecting locality is a point

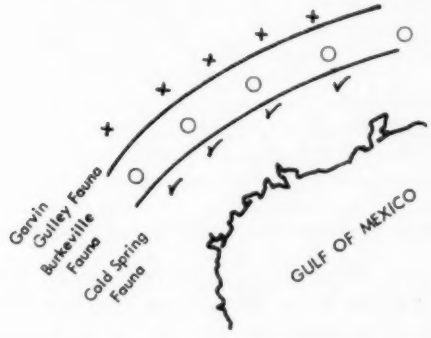

Fig. 2. Diagrammatic representation of grouping of localities containing similar fossils; biostratigraphic units.

of reference, the line drawn between is an interpolation only, subject to change with additional collecting, just as a contour line is subject to change with more precise elevation data. I now have on the diagram (fig. 2) a sequence of biostratigraphic units. They do not illustrate lithology and do not illustrate time.

The lithologic units (fig. 3) found in this same area are the Oakville

Sandstone Formation and the Fleming or Lagarto Formation. The lithology of the Oakville interfingers to the east with lower Fleming lithology so that an arbitrary cut off is necessary. The top of the Fleming Formation is covered by Pleistocene deposits.

In the central part of the area the Garvin Gulley fauna coincides by accident of occurrence with the Oakville Formation but the fauna can be extended eastward into the lower part of the Fleming Formation independent of the lithology. The Burkeville and Cold Spring faunas are both entirely within the lithologic unit, Fleming Formation.

As yet I have made no interpretation of age. An observant stamp collector, given the fossils and the locality records, could probably make the map of the biostratigraphic units. The names of the beasts would not be necessary for him nor would a knowledge of the evolutionary sequence.

The strike and dip of the lithologic units on the Coastal Plain show that the rocks farthest inland are the oldest, by the law of superposition. The same law applies to the fossils that are in the rocks so here is a first interpretation of relative time. The Garvin Gulley fauna is older than the Burkeville and it, in turn, is older than the Cold Spring Fauna.

Fig. 3. Diagrammatic representation of lithologic units.

A further interpretation can be made by comparing each of these faunas to others whose positions in the timescale has been laboriously worked out by the same method of superposition. The Garvin Gulley fauna seems to have lived during an Age called Arikareean or Early Miocene, the Burkeville and Cold Spring faunas seem to have lived during the Hemingfordian Age or Middle Miocene. This is my *interpretation*. The fossils did not come labeled as to time. I determined their respective ages by an interpretation of several lines of evidence. The rocks containing these faunas constitute Stages so I can now draw Age/Stage boundaries on a map (fig. 4). Again, this line is no more visible in the field than is a contour line. Its position is subject to change as is

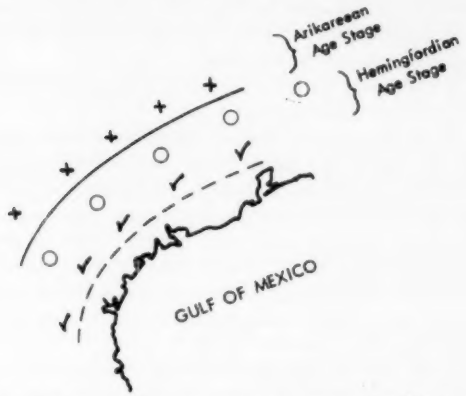

Fig. 4. Diagrammatic representation of time-stratigraphic units.

the biostratigraphic unit line between the Garvin Gulley Fauna and the Burkeville Fauna. The two here coincide, but they do not have to.

The term Arikareean Age is based on an arbitrary time during which certain mammals lived. The term Arikareean Stage is based on the rocks deposited during the time those mammals lived. These Arikareean rocks and fossils are ones that happened to be found earlier in Nebraska, that is, the type section of the time-stratigraphic unit Arikareean Stage is in Nebraska. The time, Arikareean Age, is defined as that characterized by the presence of *Parahippus*, a horse, living with certain dogs, rhinos, rodents, etc. The type section of the Hemingfordian Stage is also in Nebraska and is similarly defined from rocks and fossils overlying the Arikareean Stage. One of the keys to the recognition of the Hemingfordian Age/Stage is the sudden appearance of another more advanced horse, *Merychippus*.

The Burkeville and Cold Spring Faunas in Texas both contain *Merychippus* and enough other mammals in common with the type Hemingfordian fauna to correlate them. The Garvin Gulley Fauna has *Parahippus* but 10 out of every 100 single teeth of the same size and proportion possess the morphological character that makes them *Merychippus*. Moreover, on the Gulf Coast individual skulls of these horses have been found with some teeth in the same dentition identifiable as *Parahippus* and the others as *Merychippus*.

The Gulf Coast *Parahippus* is obviously in the process of evolving into *Merychippus*. Since *Parahippus* is absent in the Burkeville Fauna it seems obvious that it became extinct by evolution into *Merychippus*. Once *Merychippus* had evolved it migrated northward and lived alongside surviving parahippines in Nebraska.

The appearance of *Merychippus* then at the type section in Nebraska is one of migration from the Gulf Coast northward. The appearance of *Merychippus* on the Gulf Coast is by evolution and must be the earlier.

What does this do to our line between the time-stratigraphic units, now that the time-line between the Arikareean and Hemingfordian in Texas is

demonstrably older than that line in Nebraska? In another way of speaking the so-called isochronous surface between the ages/stages by the best possible paleontological evidence is not isochronous.

What now, is the significance of the Nebraska type section? I should like to examine the concept of types in stratigraphy.

That order could be made of the stratigraphic section and of the multitudinous variety of fossils has never been doubted by stratigraphers and paleontologists. It was and still is, in itself, a tremendous act of faith which we have more or less unconsciously accepted. With the scientists of the last century this faith was perhaps stronger and more certain. They proposed numerous laws, often gave them fancy Greek names, and believed these laws to be of world-wide application. With a small amount of data such interpretations could reasonably be made, and perhaps many of the so-called laws would have stood up if the amount of data had remained small.

Unfortunately, in one narrow-minded sense, a great deal more data have been accumulated since the so-called laws were proposed and one by one they have been demoted. Large amounts of data necessitated the development in the twentieth centry of statistical concepts, especially when the data included a large number of variables. Developing with statistical theory has been the theory of sampling.

Let us look back from our vantage point of today and see what has happened to these data or these samples during the development of stratigraphy, and paleontology and biology. By a long series of events in history these sciences began in England and Western Europe. Classifications of rock and of animals, both living and fossil were begun by a few brilliant men living in this part of the world.

A classification of animals and plants was invented and quickly adopted. It was proposed by Linnaeus who at that stage in his career believed in the fixity of species. A classification of sedimentary rocks, the time scale, grew out of the efforts of men who believed in catastrophism. Both of these classifications are still in use because better ones have not yet come along.

Within these independent classifications grew the *type* concept. In a local area with a small amount of data there were different and distinct types of animals and of sediments. The early classifier either of rocks or animals accepted as axiomatic that the things he was classifying conformed to a "type".

To quote from Mayr, Linsley and Usinger (1953, p. 26):

> The original concept of a species, the species of the local naturalists Ray and Linnaeus, was a species without the dimensions of space and time. Such a species is always separated by a complete gap from other sympatric species. In its purest form it is clear-cut and has objective criteria, because it is defined by the gap that separates it from other sympatric species. This local species is the yardstick [type, JAW] by which all other situations are measured. Lacking the dimensions of space and time such a species is not evolving, it is static. It is for this reason that the nondimensional species has a great deal of objectivity and can be defined unequivocally (Mayr, 1949).

Concepts of time units in stratigraphy were early arrived at in a similar way. To quote from Dunbar and Rogers (1957):

> Fuchsel divided the Flötzgebirge of Thuringia into 9 rock units which he called series, 6 of which had subordinate basal units . . . Then, after objectively describing these rock units as such, he proceeded to erect precisely corresponding time units . . . .

The time units used by Fuchsel were easily distinguishable locally because they were defined by just those local gaps or lithologic changes that separated the rock units.

This equating of time units with type sections of rock has been explicit or implicit in all stratigraphic codes and is present in some of the modern textbooks. To quote from Moore (1958) p. 135:

> Like the Cambrian, the rocks called Ordovician have their type locality in Wales. . . . The section represented by outcrops there furnished *the* (italics mine) standard for comparison with deposits of similar age elsewhere in Europe and on other continents, *even though* (italics mine) many features of the Ordovician record are more simply and fully recorded elsewhere.

This is an extreme statement and Moore admits elsewhere, p. 27, that "the span of *some* (italics mine) divisions is modified by data derived from other parts of the world.

I am fully aware that a biologic type specimen is not the same as a type section. You can lop a few feet off the top or bottom of a section and still have a section, but it is not considered good technique to lop off the head or tail of an animal, or to break pieces off a type ammonite if it doesn't look like it is supposed to. Even so the thinking behind typology in biology and in stratigraphy leads to strikingly similar conclusions.

As Mayr points out, the nondimensional species has a great deal of objectivity and can be defined unequivocally. Both of these are highly desirable attributes. They make taxonomy easy; compare an unknown with the type—if it matches, label it by that name, and file it in a drawer. If it doesn't match, give it a new name. It is separated from other species of the same local area by a complete gap and its limits are defined by that local gap. Such species are static and nonevolving. With the accepntance of evolution the gaps between species had to go, and when the gaps went those comforting attributes objectivity and unequivocalness went too.

Stratigraphic typology leads to similar dead end conclusions but it offers the same tempting attributes—objectivity and unequivocalness. The logic of typologic stratigraphy proceeds like this. In the type section fossil zones A, B and C are found. One hundred miles away fossil zones A and C are found to be continguous, zone B is missing. The typological conclusion is that there is an unconformity between zones A and C. If no evidence for an unconformity can be found in the field there is now an easy out, because it can be said that there is a paraconformity between them. This takes care of the hiatus typologically called for by the absence of zone B. Our objectivity and unequivocalness are preserved and our new section is really, after all, like the type.

The end of this is that the rigid inflexible stamp of the type must be imposed throughout the world. If the type marine Triassic is divisible into three parts then the continental redbed section must be also, even though an unrecognizable disconformity must be placed in the section to create a hiatus in order to account for a reputedly absent Middle Triassic (Moore, 1958, p. 336). The alternative, that the European vertebrates that lived while the Muschelkalk was being deposited did not get to western North America, is not considered. Good typology assumes that they did get to North America and lived exactly during the time when an unrecognizable disconformity was being formed.

I can not claim that there are no occurrences where a Zone B is absent because of erosion, nor can I claim that there is no such thing as a paraconformity, but this method seems too glib. Paraconformities should be a last resort when all means of positive explanation fail. And unrecognizable disconformities should also.

To return to our analogy between the classifications of biology and stratigraphy: The acceptance of evolution meant that biologic taxonomists were in fact dealing with a continuum, life; from its earliest forms to its living forms, from its simplest to its most complex, one inconceivably vast complex of closely or distantly related forms. The modern concept of a species then became multidimensional moving, and dynamic but, alas, necessarily lost much of its objectiveness.

The type specimen in a modern concept of a species (Simpson, 1940) plays a different role, an important one but not much more so than that of other museum specimens of the same species. With a species now thought of as a population, the type specimen is just a single sample of this population. All other collected specimens are also valid samples and from this total known sample, inferences are drawn which form the basis of the greater concept that is the species. The totality of a species is really unknowable. How could you tell whether or not you had shot the largest possum? or the smallest? Even the species of whooping crane is really unknowable because we can not examine those that were living 100 years ago.

A type then in modern taxonomy is a sample; a sample that is useful as the basis for a description, useful as a fixed point to attach a name and that is about all. For comparison an unknown must be compared not only with that part of the sample known as the type but with all other known samples of the species, or at least a statistically representative sample.

Is not a type section in stratigraphy also a sample? Are not the Cretaceous rocks of England but a sample of the sum total of Cretaceous rocks deposited? Is not the type section of the Austin Chalk just a sample of a larger three dimensional lithologic unit? A sample of the Austin Chalk near Dallas is just as valid as samples found at San Antonio or Austin in furnishing the concept, Austin Chalk Formation. After all the type section is only a one dimensional sample of this three dimensional thing called Austin Chalk Formation. The formation should be a concept based on inferences drawn from all known samples, both surface and subsurface. We can never observe all of the formation, Austin Chalk, any more than we will ever observe all of a species. The Austin Chalk would have to be floating in the air so we could see the bottom of it but not too high but what we could jump on it and observe the top. Its boundaries over practically all of its extent will remain vague since we can observe only a minute fraction of the Austin Chalk Formation.

Hedberg (1958, p. 1885) argues:

> An equally critical point, which continually seems to need emphasis, is that all types of stratigraphic units require actual *type* or *reference rock sections* for adequate definition and that without these and adequate descriptions, they and their boundaries will always be vague and controversial if not actually meaningless.

With the necessity for type *and* reference rock sections I agree, but I can not see that they in themselves clearly fix the position of a boundary ten miles

away. Because the fish I caught last Saturday weighed one pound and the type weighs two pounds does not mean they are not both white bass.

Formation boundaries are not always observable. In a gradational change from one lithology to another it is common practice to place an arbitrary boundary. But an arbitrary boundary is not observable. If it were observable it would not have to be arbitrarily placed.

Like biology, stratigraphy is also dealing with a continuum. Stratigraphy has the continuum, time, which has been divided into continguous time units. It also has the continua deposition and erosion, both continuous in time but discontinuous geographically. Nondeposition is similar in both respects.

Time of course has no type section. But time-stratigraphic units do. Must we still stamp a rigid, inflexible conformity on all stratigraphy? Can we finally break away and recognize that fossils in themselves do not magically tell us accurate time? We are now dealing with data that can prove this. An appeal to what is found at the area that was accidentally described first will no longer work. Let us recognize type sections for what they really are, samples, useful samples, but still just one of many known samples of rocks and fossils. Let us realize that there is no easy, objective, unequivocal way to classify the products of processes that are continuous.

The present terminology of the Ashley Code is sufficient to deal with most problems of stratigraphy. What is needed most is a change in thinking, a realization that deposition and erosion, and nondeposition are dynamic processes going on all the time. These processes used to be thought of two dimensionally instead of four dimensionally. Stratigraphy needs to shake off the bonds of typology that leads only to sterile repetitious description of the same things in different areas.

A different way of expressing the nomenclature of stratigraphy is shown here.

Time Unit		Time-stratigraphic Unit	Composed of Rock Units
Era			Group, Formation, Lentil, etc.
Period	greater than ——	System	and
Epoch		Series	Biostratigraphic Units
Age		Stage	Zone or Fauna

To return to the Gulf Coast Miocene faunas, by interpreting the Garvin Gulley Fauna as Arikareean, I mean that this fauna is most nearly like that found not only in the type section but in other sections determined by my colleagues as Arikareean, and that my inclusion of the Garvin Gulley Fauna into the Arikareean stage modifies somewhat but does not destroy the concept of Arikareean as held by my colleagues. To place it above or below would certainly destroy the concept of those Ages/Stages. To create a new one is unnecessary.

It is the necessity of communicating ideas that forms the stabilizing influence once typology is relegated to its proper place. My assignment of the Garvin Gulley Fauna to Arikareean implicitly carries with it the sense that

part or all of the type Arikareean is correlatable with my section. I could assign my biostratigraphic units to the Waucoban, but I would have a very difficult time conveying the idea Early Miocene with that word.

The clear-cut objectiveness, the unequivocalness, is gone. The isochronous surface is not traceable, but I think stratigraphy can gain a more flexible, workable, and more accurate approach to what is found in the field. Not just in the field at the type area but all over the world. Let stratigraphic taxonomy catch up with biologic toxonomy.

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ERRATA

In the article by George C. Kennedy, "Phase relations in the system Al_2O_3—H_2O at high temperatures and pressures," (this Journal, October 1959, p. 570) the boundary on figure 3 labelled Kaolin/Pyrophyllite + Sillimanite + H_2O should be labelled Kaolin/pyrophyllite + corundum + H_2O.

In the article by E. F. Osborn on the "Role of oxygen pressure in the crystallization and differentiation of basaltic magma" (this Journal, November 1959), figures 7 and 10 were inadvertently interchanged by the printer. The diagram on page 631 is figure 7 and should appear on page 625; that on page 625 is figure 10 and belongs on page 631.

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